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ACTA RADIOLOGICA

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QUELQUES OBSERVATIONS SUR LA CHONDROMATOSE CAPSULAIRE ET SUR SES CARACTERES RADIOLOGIQUES

par

H. J. Panner

(Tabulæ XXII—XXIV)

Si nous désirons faire ici une communication sur la chondromatose capsulaire, c'est en premier lieu parce que, jusqu'à présent, cette affection nous semble ne pas être assez généralement connue; et, deuxièmement, parce que, le plus souvent, le diagnostic en pourra probablement être établi au moyen d'un examen radiologique. C'est surtout cette dernière question que nous allons étudier ici.

Qu'il nous soit cependant permis de préfacer cette étude de quelques brèves observations sur la nature même de l'affection dont il s'agit.

Reichel fut le premier qui en décrivit un cas (dont le siège était au genou), et qui en précisa, en même temps, le caractère comme étant celui d'une affection chondromateuse de la capsule. C'était en 1900. Plus récemment, Mueller, Riedel, Lexer, ainsi que deux collaborateurs de ce dernier: Rehn et Eden, ont signalé chacun un cas. Une communication de Troell, relatant deux cas, fut pendant longtemps la seule, à ce sujet, des pays Scandinaves. Tandis qu'ainsi la plupart des observations on trait à un cas, ou deux seulement, il existe une communication, de Jones, de la clinique Mayo, qui ne comprend pas moins de dix-neuf cas; et au Congrès des Chirurgiens de Pays du Nord, qui eut lieu en 1925, à Copenhague, M. Fenger (du Rigshospitalet, Service A; Chef: le Professeur Schaldemose) en soumis quatre, lesquels nous eûmes l'occasion de diagnostiquer dans le département de radiologie du dit hôpital. De nos propres observations, nous pouvons y ajouter encore trois autres. Ce n'est donc, au total, qu'une cinquantaine

de cas qui ont été signalés; par conséquent, on peut dire qu'il s'agit d'une affection assez rare. Toutefois, il nous paraît probable que, du moment où ses caractères distinctifs auront été plus généralement reconnue, l'on verra que la chondromatose capsulaire est réellement plus fréquente qu'on la croit aujourd'hui. Les dix-neuf cas de JONES, et le fait que nous en avons nous-mêmes observé sept cas en très peu d'années, nous semble une très forte indication dans ce sens-là. D'autre part il est probable que, dans pas mal de cas, un diagnostic erroné ait rattaché les symptômes de ce qui fut en réalité une chondromatose capsulaire, à quelque condition pathologique différente; le plus souvent, probablement, à une arthrite déformante aux arthrophytes nombreuses.

Nous n'allons pas nous attarder ici aux détails pathologiques ou cliniques de la chondromatose. Nous ferons seulement remarquer que la plupart des auteurs paraissent être d'accord pour désigner la synovie comme étant l'endroit où cette affection prend sa naissance; de sorte qu'on peut la rencontrer tant dans les articulations que dans les bourses séreuses et les gaines synoviales. C'est surtout aux points où une synoviale se joint soit au cartilage articulaire, soit au périoste — et où, de l'état fœtal, il reste encore normalement des groupes de cellules cartilagineuses — que naissent des tumeurs cartilagineuses, dans lesquelles il se produit graduellement des calcifications, ou des ossifications, plus ou moins étendues. Mais la production de tumeurs cartilagineuses absolument identiques, de nouvelle formation, peut avoir lieu, également, dans toutes les autres parties de la synoviale, des cellules synoviales mêmes. Le degré de leur développement est variable: parfois la cavité capsulaire sera complètement remplie de tumeurs plus ou moins grandes — adhérant à la capsule en grappes plus ou moins serrées — et de corps flottants, de structure histologique en tout point analogue, dont le détachement est dû à la circonstance que, pendant leur croissance, ils ont pénétré de plus en plus loin dans la cavité articulaire, jusqu'à ce que le pédoncule de plus en plus aminci, par laquelle ils adhéraient encore à leur base, a fini par être déchirée. L'accroissement, par prolifération, des tumeurs individuelles peut continuer jusqu'à ce que, parfois, elles atteignent les dimensions d'un œuf de poule. Elles peuvent même — quoique assez rarement — émerger de la capsule et pénétrer dans les tissus périarticulaires et musculaires; leur pression résultant à une atrophie et ainsi amenant la diminution des os de l'articulation, les envahissant — parfois jusque dans le tissu spongieux — et s'y enkystant, sans toutefois s'y infiltrer. D'après LEXER, l'affection doit être considérée comme une véritable tumeur capsulaire, bénigne; elle

n'est pas susceptible de métastase. De l'absence de tout processus inflammatoire il s'ensuit qu'il n'y a aucun exsudat de la capsule non plus; si toutefois il s'en produit, il faudra le regarder comme un phénomène secondaire, de même que l'arthrite déformante peu prononcée qu'on observe parfois, mais dont le degré de développement est toujours très faible par comparaison à celui de l'autre affection.

L'affection est le plus souvent localisée à l'articulation du genou; ensuite à celle du coude; plus rarement elle se manifeste dans les articulations scapulo-humérales ou coxales, ou dans celles des carpes ou des tarses. Nous avons déjà dit qu'elle peut frapper aussi les gaines synoviales et les bourses séreuses, soit seules, soit combiné avec une affection identique de l'articulation la plus proche de la bourse attaquée. Très rarement on a vu l'affection porter sur plusieurs articulations d'un même individu; mais le plus souvent elle est monoarticulaire.

L'étiologie de la chondromatose capsulaire — comme celle de la plupart des tumeurs — est absolument inconnue. Il va sans dire que, dans l'anamnèse de beaucoup de ces cas, l'on trouvera le récit de quelque traume ou autre, dont le rapport direct avec l'origine de l'affection doit néanmoins être considéré comme excessivement problématique.

Les symptômes consistent naturellement dans la diminution plus ou moins considérable des mouvements de l'articulation lésée, et dans l'enflure de tissus qui l'entourent. Dans cette partie enflée on constate facilement la présence des différents éléments qui la composent: fréquemment sous la forme de corps mobiles, qui provoquent assez souvent des enclenchements.

Le diagnostic, dans la majorité des cas, sera probablement aisé, surtout quand il s'appuyera sur les données radiographiques.

L'image radiographique présentera un aspect très différent, tout selon l'étendue de la lésion et selon le degré atteint par la calcification, ou par l'ossification; celles-ci étant les facteurs que rélèveront les skiagrammes, tandis que les tumeurs cartilagineuses elles-mêmes n'y seront pas directement visibles. C'est aussi pourquoi l'examen radiologique, si tant est qu'il soit fait dans le stade initial de la maladie (époque à laquelle on est rarement appelé à le faire, du reste; vu que les symptômes de l'affection se déclarent généralement assez tard dans le cours de son évolution) ne donnera qu'un résultat négatif. C'est seulement quand la calcification aura eu lieu, que des ombres plus ou moins étendues, et d'opacité variable, commenceront à paraître; aux plis synoviaux d'abord, puis, peu à peu devenant plus larges et plus précises, et prenant le caractère de formations dont on distingue nettement les limites, mais qu'on ne pourra guère

différencier de corps flottants ayant peut-être une origine tout à fait autre, à cause de la similarité qui existe largement entre les structures histologiques des deux. Dans des cas graves, où les tumeurs ont, par leur pression, causé l'usure plus ou moins prononcée des os, l'on pourra observer les défectuosités dans ces derniers; sans que l'image radiographique ne présente pourtant un aspect semblable à celui qu'on observe dans le cas d'un véritable ostéo-sarcome. La défectuosité se présente régulière, nettement circonscrite; mais il n'y a aucun envahissement infiltratoire dans le tissu osseux. Nous avons déjà dit qu'aucune arthrite ne forme le point de départ de cette affection, ni en est nécessairement un corollaire. Les petits ostéophytes que l'on observe souvent aux angles des articulations doivent, par conséquent, être considérés comme des complications secondaires: l'expression d'un état d'irritation constante causée par la présence des corps étrangers; mais qui souvent, par leurs faibles dimensions mêmes, contrastent d'une manière frappante avec l'énormité des altérations qui caractérisent autrement l'image radiologique de la lésion.

C'est là, précisément, que nous voyons une indication précieuse pour le diagnostic différentiel de cette affection avec l'arthrite. Si dans l'image radiographique l'on reconnait tous les signes caractéristiques d'une grave arthrite déformante, on n'osera pas s'arrêter à aucune autre définition, encore qu'on serait dans la présence d'un nombre considérable de corps mobiles; à moins que le fait d'une cavité capsulaire absolument bourrée de ces corps vous fera peut-être hésiter devant la décision, à cause de la rareté même d'un tel phénomène dans le cas d'une arthrite déformante. D'autre part, la présence de pareilles altérations chez des individus entre les âges de 20 et de 40 ans doit faire soupçonner une chondromatose; d'autant plus que, de cette maladie, les attaques se produisent relativement souvent à cette période de la vie, tandis que les arthrites déformantes, sévères, débutent plus rarement à l'âge viril.

Le diagnostic différentiel de chondromatose avec un ostéo-sarcome sera probablement aisé, dans la plupart des cas. Ainsi, nous doutons beaucoup que les cinq cas décrits par KIENBOECK (dans deux communications), soient vraiment, tous, des chondromatoses capsulaires. Dans deux, la cause du décès est donnée comme »Lungenlähmung», ce qui indique probablement une métastase (aucune nécropsie n'eut lieu); et les radiographies qui accompagnent les observations ne paraissent nullement caractéristiques de la chondromatose; en effet, plusieurs d'entre elles nous semblent donner l'impression, bien davantage, de véritables ostéo-sarcomes. Pour admettre les conclusions de KIENBOECK il serait nécessaire d'établir une différence — comme il le fait, du reste — entre deux types de chondromatose: l'un bénin,

l'autre malin; ce qui compliquerait de beaucoup tout le problème. Mais pour le présent il sera légitime de considérer la chondromatose capsulaire, dans son ensemble, comme une maladie nettement définie, à marche bénigne; aucune des observations publiées ayant fourni des signes de sa malignité, bien que, dans certains cas, il y ait eu une récurrence locale. Dans un cas seulement, rapporté par JONES, l'affection capsulaire était compliquée d'un chondro-sarcome du fémur.

Voici quelques observations.

Le cas qui, tout le premier, fixa notre attention à cette maladie ne fut pas observé par nous personnellement. Nous le devons à M. le Dr HASLUND, médecin-en-chef de l'hôpital de Hoeng, où ce malade avait été traité sous sa direction. M. HASLUND a bien voulu nous communiquer ses observations et mettre à notre disposition les radiographies prises par lui.

Il s'agissait d'un agriculteur de 39 ans. En 1910, il avait reçu un coup de pied d'un cheval, qui, l'atteignant au genou gauche, l'avait rendu invalide pendant six mois; mais, passé cette époque-là, il avait de nouveau joui de sa pleine capacité de travail. En 1921, il s'est cassé la cuisse gauche, et l'examen de cette lésion a découvert une tuméfaction considérable de l'articulation du genou, avec des tumeurs solides à l'intérieur de l'articulation même. Dans les skiagrammes (fig. I et II) l'on voit l'articulation entière — tant la partie en bas de la rotule que celle qui correspond à la bourse sous-crurale — ainsi que l'espace en arrière de la partie inférieure du fémur et la partie supérieure des os de la jambe, remplis par des ombres, dont quelques-unes grandes comme des noix, ressemblant de beaucoup à celles que produit la présence de corps flottants, calcaires ou osseux. Ces ombres se confondent presque partout, de sorte qu'il est impossible, sauf dans de rares endroits, de bien démêler les contours des silhouettes individuelles. Il est raisonnable de supposer que les corps responsables de ces ombres se trouvent tant dans l'articulation même que dans les bourses séreuses à côté. A juger par le contour inférieur de la rotule, et par celui de la tubérosité interne du tibia, on dirait que les tumeurs aient, par leur pression, causé l'usure des os, dans lesquels on observe comme de petits creux à ces endroits-là, sans qu'il y ait rien de pareil nulle autre part. L'interstice articulaire est normal, et c'est seulement aux tubérosités, externe et interne, du tibia qu'on observe une faible formation nouvelle de tissu osseux.

Une *arthrotomie*, faite par le Dr HASLUND, révéla la présence, dans l'articulation du genou, d'un très grand nombre de corps durs, noduleux, de toutes les formes, et de dimensions variables: depuis celle d'un petit pois jusqu'à celle d'un noix. Ils adhéraient tous à la capsule articulaire si solidement qu'on dut se contenter d'en enlever un seul, aux fins d'un examen microscopique. Cet examen — fait par le Professeur OLUF THOMSEN — montra que le tumeur était composé de pur cartilage hyalin, sans le moindre signe de malignité.

L'ensemble des signes objectifs, l'image radiologique avec les nombreuses formations d'ombres qui correspondaient partout avec l'étendue de la capsule, l'absence d'une véritable arthrite déformante,

— tous ces éléments, avec les observations pathologiques et histologiques, doivent être considérés comme typiques de la chondromatose capsulaire.

Ce n'est pas dans tous les cas, pourtant, qu'on trouve dans l'image radiologique des altérations aussi prononcées que nous venons de les voir ici. Si la calcification n'est que peu avancée, on n'observera — comme nous avons déjà dit — que très peu qui soit anormal, tel que le démontrent les skiagrammes du cas suivant:

Il s'agit d'un agriculteur, âgée de 30 ans, entré au Rigshospitalet, dans le service D (le Professeur SCHALDEMOSE).

Trois ans et demi avant son entrée, il avait reçu, d'un cheval, un coup de pied sur le côté médial du genou droit, dont il ne ressentit pourtant que quelque incommodité passagère. Un an, à peu près, après l'incident, une tuméfaction commença de se déclarer à l'endroit de la lésion, sensible et accompagnée de douleurs; en même temps que la mobilité du genou se trouva plus ou moins gênée. Il n'y avait aucune indication de corps flottants; mais l'articulation était légèrement gonflée, et en palpant on pouvait se rendre compte de l'existence, du côté médial de la partie supérieure du tibia, d'une tumeur qui paraissait faire corps avec l'os, ainsi que d'épaississements, çà et là, correspondant à la capsule.

Dans les *skiagrammes* (fig. III et IV) l'on voyait autour du genou, dans le circuit de la capsule, de petites ombres vagues, seules ou par groupes, qu'on dirait des foyers épars de calcification. A la partie médiale du tibia, à l'endroit où se trouvait la tumeur dont nous venons de parler, l'on voyait une petite excavation dans l'os, dont le contour était néanmoins, à cet endroit, parfaitement bien défini. Autrement, l'image radiographique du genou n'offrait rien d'anormal. A notre avis, le diagnostic devait porter sur une tumeur capsulaire: un chondrome ou bien, peut-être, un sarcome avec des calcifications.

L'examen d'un échantillon excisé démontra la nature chondromateuse du produit pathologique; et quand une arthrotomie fut ensuite pratiquée, la capsule entière se révéla couverte de tumeurs, grandes et petites, dont celles qui étaient placées à la proximité des plis synoviaux ne se laissaient que difficilement séparer de la structure osseuse, dans laquelle elles avaient pénétré, s'y faisant des creux, sans pourtant s'infiltrer dans le tissu de l'os.

Dans ce cas aussi, l'ensemble des phénomènes observés constitue un tableau nettement typique de la chondromatose capsulaire; et nous ne doutons pas que, si à cette époque-là nous aurions eu, à ce sujet, les connaissances que nous croyons posséder aujourd'hui, nous aurions probablement diagnostiqué le cas dans ce sens-là, sans hésiter.

Si l'affection, telle que la montrent les skiagrammes, se trouve localisée — soit entièrement, soit, au moins, très largement — à quelque bourse séreuse, avec des formations d'ombres typiques, comme cela se voit, par exemple, dans le cas — d'une femme âgée de 45 — représenté par fig. V, alors le diagnostic d'une chondromatose capsulaire sera probablement le seul raisonnable. Nous voyons là une série de silhouettes, dont quelques-unes larges comme des

cerises, d'opacité variable, ressemblant à celles que produisent des corps flottants, et dont la situation correspond à celle de la bourse semi-membraneuse. En même temps, le genou n'offre rien qui soit anormal, sauf des formations insignifiantes d'ostéophytes, et, juste avant l'interstice articulaire du genou, une ombre, de la dimension d'un pois, et d'une apparence pareille aux ombres les moins opaques parmi celles qu'on observe en arrière de l'articulation.

Une opération, pratiquée par le Dr P.-K.-J. Bentzon, démontra qu'il s'agissait, en effet, d'une chondromatose capsulaire. —

Nous ne voulons pas abuser de la bienveillante attention du lecteur en rapportant d'autres observations encore. Nous nous bornerons donc à ajouter simplement les skiagrammes de quelques autres cas appartenant à la même catégorie de lésions. Le numéro VI est tellement typique que le diagnostic ne peut pas faire de doute, encore que, dans ce cas, il manque la confirmation que seule peut donner une intervention chirurgicale. Dans le cas représenté par les numéros VII et VIII — et qui était celui d'une femme, âgée de 70 ans, qui souffrait depuis de longues années de « rhumatisme » du genou — aucune opération n'eut lieu non plus; mais le très grand nombre de corps flottants autour du genou nous portent néanmoins à croire que, là aussi, le diagnostic de chondromatose capsulaire serait, en toute probabilité, le plus raisonnable. En tout cas, on fera bien, dans des cas pareils, de ne pas perdre de vue la possibilité qu'il s'agisse de cette affection. Ces deux représentations ont, du reste, une très grande ressemblance aux numéros IX et X — le cas d'une autre vieille, âgée de 65 ans, et également souffrante depuis des années, dans lequel notre diagnostic de chondromatose fut ensuite confirmé par l'opération pratiquée.

Si la question se présente d'une intervention chirurgicale, il est important de savoir d'avance si le cas est vraiment celui d'une chondromatose capsulaire ou non; car, le plus souvent, la chondromatose est excessivement difficile à enlever. Pour être raisonnablement sûr qu'elle ne reparaisse pas, il faut que la capsule soit enlevée complètement; ce qui évidemment, dans beaucoup de cas, présentera de grandes difficultés, sans parler des résections très étendues qu'a parfois nécessitées une opération pareille.

Il s'agit donc de bien peser, dans chaque cas, si l'incommodité, la gêne résultant pour le malade de cette affection, est vraiment assez grande pour justifier une opération de cette ampleur. Souvent les symptômes seront si faibles que le malade refusera probablement de se laisser opérer; parfois, quand le nombre des corps flottants n'est pas très grand, le traitement le plus approprié consistera pro-

bablement à les enlever, tous ou en partie. Mais même le fait de l'enlèvement assez minutieux de la capsule entière ne fournit pas une garantie absolue de ce qu'il n'y ait récidive, plus tard.

RÉSUMÉ

L'auteur fait brièvement la revue des opinions diversement tenues sur la nature de la chondromatose capsulaire; puis, il donne la description de l'image radiologique typique de cette affection, avec les formations d'ombres — correspondant aux tumeurs cartilagineuses plus ou moins calcifiées ou ossifiées — qu'on trouve souvent dans toutes les parties de la capsule, ainsi que dans les bourses séreuses attenantes, et les défectuosités nettement limitées qu'on observe dans les os, et qui sont les résultats de l'usure produite par la pression des tumeurs pénétrant dans le tissu osseux. Il note exprès l'absence de toute arthrite déformante; aux angles de l'articulation seulement on observe assez souvent de petites ostéophytes, qui doivent être considérées comme des formations secondaires, dues à la présence, dans l'articulation, de tant de corps étrangers.

Le plus souvent, les données radiologiques, jointes à l'observation des symptômes cliniques, rendront aisé le diagnostic différentiel de la chondromatose capsulaire avec ces autres affections: l'arthrite déformante et les ostéosarcomes, avec lesquelles il pourrait y avoir question, autrement, de la confondre.

L'auteur est d'opinion que la chondromatose capsulaire, une fois que les caractères typiques de cette affection auront été plus généralement réalisés, sera en toute probabilité reconnue comme étant bien moins rare qu'on ne l'a crue jusqu'à présent.

SUMMARY

The author reviews the different opinions held concerning the nature of the capsular chondromatosis; he then describes the typical roentgenological appearance of that condition: with formation of opacities — corresponding to cartilaginous tumours more or less calcified or ossified — frequently found in all parts of the capsule as well as in neighbouring serous bursae. He further describes the clearly defined defective areas in the bone, the results of the erosion produced by the pressure of the penetrating tumour. He notes particularly the absence of any deforming arthritis; only in the angle of the joint does one fairly often find small osteophytes which should be considered as secondary formations due to the presence of too many foreign bodies in the articulation.

In most cases the roentgenological findings together with the clinical symptoms will render easy the differentiation of the capsular chondromatosis from those other conditions, atrophic arthritis and osteo-sarcoma, with which it may otherwise be confounded.

The author is of the opinion that the capsular chondromatosis, once its typical characters have become more generally recognised, will in all probability be found to occur much less rarily than at present believed.

ZUSAMMENFASSUNG

Der Autor gibt einen kurzen Überblick über die verschiedenen Ansichten betreffs der Natur der Kapselchondromatose; dann beschreibt er das typische radiologische Bild dieser Affektion mit den Schattenformationen, die man — entsprechend den mehr oder weniger verkalkten oder verknöcherten Knorpeltumoren — häufig in allen Partien der Kapsel sowie in den ihr zugehörigen serösen Taschen findet, und mit den in den Knochen zu beobachtenden scharf begrenzten Defekten, die durch die usurierende Wirkung des Druckes der in das Knochengewebe penetrierenden Tumoren erzeugt werden. Er weist ausdrücklich auf das Fehlen jedes Anzeichens von Arthritis deformans hin; nur an den Gelenksrändern beobachtet man nicht selten kleine Osteophyten, die als sekundäre Formationen betrachtet werden müssen, als Folgen des Vorhandenseins von so vielen Fremdkörpern im Gelenk.

Meistens werden die radiologischen Befunde, zusammen mit der Beobachtung der klinischen Symptome es leicht machen, zwischen Kapselchondromatose einerseits und anderseits den Affektionen, mit denen sie sonst ev. verwechselt werden könnten, nämlich der Arthritis sicca sowie den Osteosarkomen, die Differentialdiagnose zu stellen.

Der Autor ist der Ansicht, dass sich die Kapselchondromatose, sobald die typischen Sondereigenschaften dieser Affektion einmal allgemeiner bekannt sein werden, höchst wahrscheinlich als weit weniger selten erwiesen wird, als man es bis jetzt geglaubt hat.

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[1] La contribution de cet auteur contient une bibliographie détaillée.

ON THE DEMONSTRATION AND ANALYSIS OF CALCANEO-NAVICULAR COALITION BY ROENTGEN EXAMINATION

by

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(Tabulæ XXV—XXVI)

In 1920, I laid before the congress of The Scandinavian Orthopedic Association in Copenhagen a communication concerning a series of cases, 5 in all, in which I had proved, by roentgen examination, the presence of an abnormal, congenital continuity between the anterior extremity of the calcaneum and the navicular bone — an anomaly already at that time well known as a mere anatomical find, but never before diagnosticated clinically or in vivo. The anomaly must be interpreted as due to the presence of an os calcaneum secundarium, situated between the above-named two bones and in fibrous or osseous connection with both. — Clinically, all my cases presented the symptoms of flat foot, and in the three adult individuals there existed moreover a very marked, even monstrous flat-foot deformity. In a boy of 13 years the deformity was not very pronounced, and in a girl of 10 it had not (yet?) developed at all — but nevertheless a temporary pain and swelling was observed.

The flat-foot deformity cannot after this be supposed to be a primary condition, at any rate certainly not in every case of coalition; and it is, with so scanty a material, impossible to ascertain if upon the whole it will develop in every case of this kind. According to the anatomical investigations by Fitzner this should *not* be the case. At any rate I wish strongly to emphasize that the flat-foot must generally be expected to be less developed or even missing in children and very young persons, under these circumstances.

For the development of flat-foot deformity in consequence of the calcaneo-navicular coalition I have set up the following theory which I still insist upon. I hold that this deformity — which, even if not an absolute consequence, is certainly not an accidental coincidence either — is produced by the manner in which the pressure of the body-weight, acting with its anterior component[1] downward and forward through the collum and caput tali and so transmitted to the navicular bone, is, by the abnormal fixation of this bone, prevented from transmitting itself further forward; and that therefore it must act as a downward pressure on the naviculare and on the upper medial corner of the anterior extremity of the calcaneum connected therewith. Hereby this anterior extremity and the navicular bone are displaced downward, and together with the calcaneum in its entirety are turned inward, in pronation.

My paper »On Coalitio calcaneo-navicularis» was subsequently published in The Journal of Orthopedic Surgery for 1921, to which publication I must refer with regard to details and literature. I should only wish to emphasize here somewhat more strongly than in the foregoing publication, that the coalition is met with in three different forms or degrees: as an amphiarthrosis between abnormal prolongations from the two bones; as a broad fibrous syndesmosis between such prolongations, often containing one or more osseous nuclei — ossa calcanea secundaria — imbedded in the fibrous tissue; and, finally, as a complete osseous coalescence, a synostosis, between them. I do not think that any distinct symptomatic difference exists between these forms, but an amphiarthrosis of small extent must in all probability be supposed less likely to produce severe pain and deformity than a complete osseous coalescence. — By the roentgen examination, conducted after the method to be described in the following, these forms may be differentiated — and especially the two first-mentioned from the third.[2]

It is obvious that this coalition, which within the space of some few years I have substantiated in 5 cases, cannot be of rare occurrence, and this is also in accordance with the earlier, merely anatomical investigations. It, therefore, seems strange that its occurrence should never before have been diagnosticated in the clinic. The simple reason probably is that it has been overlooked, an opinion also frankly expressed by our very experienced radiologist

[1] The posterior component is directed backward and downward through the corpus tali and corpus calcanei to the tubercula calcis, which rest upon the ground.

[2] The difference between the amphiarthrosis and the syndesmosis is perhaps more anatomical than clinical; but pro tem. there seems to me to be justification for its maintenance.

the late Professor J. Fr. Fischer, when I brought the subject forward at a meeting of the Danish Radiological Society. A further indication in the same direction I found, shortly after I had made my first publication, in a paper by Gelinsky: On the Os Vesalianum tarsi (Fortschritte d. Röntgenstrahlen 1904—5). He reproduces there the radiograms from a case in which he claims to have found these Ossa Vesaliana, and he adds this remark: »An den Tarsalknochen sind keine Besonderheiten.» It was therefore of great interest to me to see that the radiograms show the presence of an unquestionable calcaneo-navicular coalition in both feet.

Since my publication the coalition is not rarely diagnosticated, at least here in Copenhagen. Thus, Dr. Panner, chief of the Radiological Department at the Rigs-Hospital, has kindly informed me that in the course of some few years he found at least three such cases. — But on the other hand the danger now presents itself that the coalition may be diagnosticated on insufficient evidence, not giving safe information about its character nor any certainty of its real existence.

The Revue d'Orthopédie, 1923, contains a Mémoire by Prof. Nové-Josserand (Lyon): »Formes anatomiques du pied plat», in which that author, after mentioning the »pied plat simple» and the »pied plat congénital», refers to my paper about the coalition and in that connection gives two cases of his own observation, both in very young persons (13—17 years old), as typical examples of calc.-navicular synostosis. His very interesting paper is illustrated, not by direct reproduction of the roentgenograms, but by linear drawings from the latter. — Judging from those drawings I venture to express the opinion that no doubt the coalition is present in both cases, but that certainly there is no real synostosis in either of them. As the typical picture of a real synostosis seen in medio-lateral projection I would point to Fig. 1 (Plate XXV), representing one of my cases, in which the osseous bulk of the ant. extremity of the calcaneum is seen in direct continuity with the naviculare, forming a broad bridge between them. Nothing like this is seen in any of Nové-Josserand's radiograms (see, for comparison, Fig. 2 in the text). — In the lateral view it will be noticed that they both present a distinct prolongation of the anterior and upper corner of the extr. ant. calcanei, the »apophyse calcanéo-scaphoïdienne» of Nové-Josserand; and the existence of this apophyse suffices for him to diagnosticate not only the coalition, but even the synostosis between the two bones. This certainly is not warranted.

On the radiograms of his own cases, as well as of mine, Nové-Josserand misses the sliding forward of talus + naviculare in rela-

tion to the calcaneum, which he usually finds in the ordinary flat-foot; and he concludes from this peculiarity, — to which he attaches great importance —, that the dislocations between the tarsal bones among themselves are very much less pronounced in the flat-foot caused by coalition, than in the ordinary flat-foot. As a further support for this opinion he describes and delineates a case of flat-foot after BAISCH,[1] in which he (N.-J.) claims to see the presence of at any rate the approach to a coalition, and in which the dislocations mentioned are almost completely missing (Fig. 2). From these observations he concludes that the occurrence of flat-foot in cases of coalition is not a mechanical consequence of the abnormal structure of the skeleton, but only a consequence of the tarsalgia caused by the anomaly; and that therefore it is only a temporary flattening of the foot, produced by reflex muscular spasm, without the characteristic and fixed osseous deformity of the ordinary flat-foot. — Basing himself upon this view he rejects my theory about the development of flat-foot deformity as the consequence of the coalition.

Without entering into any detailed discussion of this line of argument I only wish it to be remembered that the three *adult* persons among my five observations all represent even very severe cases of unquestionable, fixed, osseous, flat-foot-deformity. Furthermore, I don't think it needs any special explanation that the forward dislocation of the talus is missing in these cases of coalition. Indeed, inasmuch as the talus, in every attempt to slide forward, will meet the naviculare, and inasmuch as this naviculare is itself, by its firm connection with the calcaneum, absolutely prevented from sliding forward, the talus thus sticks between these two bones as in a vice, and a dislocation forward of the talus and naviculare in relation to the calcaneum is absolutely precluded. But the pronation of the calcaneum combined with the sinking of its anterior extremity, by which the medial arch of the foot is let down to the ground and the foot turned out in abduction, constitutes the most characteristic deformity of the severe flat-foot in my three adult cases — as indeed in every flat-foot.

I shall refrain from further dilating upon this question and will turn to the point which has given rise to these remarks, namely, the doctrine of NOVÉ-JOSSERAND about the signification of the »apophyse calcanéo-scaphoïdienne». This apophyse is seen, in his radiograms in the medio-lateral projection of the foot, in its fully developed form (Fig. 1 in the text, a reproduction of Fig. 9 by N.-J.,

[1] BAISCH: Bau und Mechanik des normalen Fusses und des Plattfusses. Zeitschr. f. orthopäd. Chirurgie, Bd. 31, 1913.

from one of his own two young cases of coalition) as well as in its rudimentary form (Fig. 2 in the text, reproduced by N.-J. after BAISCH), about which latter N.-J. declares that »le calcanéum prolonge son bord antéro-supérieur en formant une sorte de bec, qui s'étend jusqu'au scaphoïde et vient manifestement se mettre en rapport avec lui». Also *this* radiogram — which, like the other (Fig. 1) shows the skeleton of the foot directly from the side, in straight medio-lateral projection — must consequently to NOVÉ-JOSSERAND represent a coalition, even if in a less pronounced degree; and as a case of coalition he certainly makes use of it in his line of argument.

It is this doctrine, and especially the interpretation of this radiogram which I have in mind when I speak of the danger now exist-

Fig. 1.

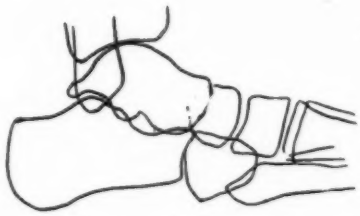

Fig. 2.

ing, that the coalition may be diagnosticated even where it does not exist.

The fact of the matter is that the radiogram of the foot-skeleton taken, as in Fig. 2, in a straight medio-lateral projection, does not at all justify the interpretation applied to it by NOVÉ-JOSSERAND. The calcanear beak in Fig. 2 is quite a normal thing; but even a more projecting beak, protruding much farther into the shadow of the naviculare, as in Fig. 1, does not with safety permit the conclusion as to a »rapport», in the sense of an abnormal contact or connection, between the anterior extremity of the calcaneum and the naviculare[1] — *because in this projection we see parts of the bones apparently in contact, which really are situated in quite different planes.* I think that this fact can be most strikingly demonstrated by pointing to the photos of the foot-skeleton reproduced in Figs. 3 and 4. In Fig. 3 the skeleton is taken in straight medio-lateral projection.

[1] Of course a certain degree of »rapport» always exists between these bones, as they are connected by the plantar and interosseous calc.-nav. ligaments.

The ant. extr. of the calcaneum is seen projecting upward with a strong beak (marked on the skeleton with a ×), which apparently is in direct contact with the naviculare — just as in fig. 2. But when the foot with the leg is turned as shown in Fig. 4, that is, with the planta inclined at an angle of about 45° to the photographic plate, and when then the rays of light (respectively the roentgen

Fig. 3.

Fig. 4.

rays) pass vertically through the interstice between the anterior part of the talus + the naviculare upwards, and the ant. part of the calcan. + the cuboideum downwards — then it is seen at once that the anterior projection from the calcaneum (marked with a ×), which previously appeared in contact with the naviculare, is really situated far from that bone and quite without any »rapport» to it.

Consequently, the interpretation of the radiogram reproduced after Baisch is not justified; there is nothing in this radiogram indicating a coalition or an unusually close connection between the calcan. and the naviculare.

The projection of the foot-skeleton used in Fig. 4 is thus shown to be the most suitable one for deciding radiographically, in case of doubt, whether a coalition is present or not, as well as for giving information about the details in the structure of that coalition. — As a matter of course the most favourable angle of inclination of the foot in relation to the photographic plate, on which it shall rest with the internal border only, must vary somewhat for the different feet; for the flattened foot this angle will be smaller than when the arch is high; and it may be necessary to experiment a little before the best result is attained.

I think that the radiographic analysis of a case of coalition which was examined in my clinic after my first publication about this anomaly, will best serve to illustrate the great importance of this inclined projection.

In Fig. 2 (Tab. XXV) is seen the skeleton of both feet in the dorso-plantar projection; the existing coalition — generally obvious, but never very distinct in this projection — is indubitable in both. About its structure in detail, however, nothing can be discerned with safety.

In Fig. 3 and 4 (Tab. XXVI) the feet are seen in the commonly used straight latero-medial projection. From Fig. 3 (left) a coalition cannot with safety be diagnosticated, even if the calcaneum-beak is remarkably strongly developed; in Fig. 4 (right) a narrow osseous bridge seems to connect the calcan. directly with the naviculare.

But it is not before we come to Fig. 5 and 6 (Tab. XXVI), taken in the above mentioned inclined projection, that we get a complete understanding of the relations, a complete view into the nature of the connection. In the left foot (Fig. 5) this connection is seen to be interrupted by an interspace with somewhat irregular borders — probably an amphiarthrosis with central cavity; a shallow, smooth excavation in the calcanear prolongation points in this direction. — In the radiogram of the right foot (Fig. 6) on the contrary, it is distinctly seen that the calcanear prolongation on this side — in contrast to the impression produced by the medio-lateral view — is in reality less developed than on the left side; but in addition it is seen that between this prolongation and the navicular bone there is situated an independent small accessory bone, a calcaneum secundarium, of somewhat irregular shape, presenting a convex border toward the calcaneum, a more straight one toward the naviculare, — presumably in tight fibrous connection with both and thus forming a solid bridge between them. — In Fig. 6 (Tab. XXVI) this detail is reproduced in the true size of the radiogram.

These pictures will, I think, demonstrate better than any ex-

planation the necessity of the radiographic examination in this inclined position of the foot, and its superiority, as compared with the commonly used positions, as a means for a safe judgment about the existence of a calcaneo-navicular coalition and about the details of its structure.

SUMMARY

The author, referring to his previous publication: »On Coalitio calcaneo-navicularis», in the *Journal of Orthopedic Surgery* for 1921, discusses the theory concerning the combination of that anatomical anomaly with flat-foot symptoms and deformity, which he sets up there and still insists upon. The author refutes the objections to his views raised by NOVÉ-JOSSERAND in a Mémoire: »Formes anatomiques du pied plat», published in the *Revue d'Orthopédie*, in 1923, in which N.-J. refers to two young cases of coalition observed by himself, and to a case of slight flat-foot observed by BAISCH, which N.-J. claims to be a case of coalition. From those three observations, N.-J. claims that the flat-foot appearing in cases of this kind is only a temporary disability produced by reflex muscular spasm. His diagnosis of the coalition in the last case is based only upon the appearance, in a radiogram taken in straight medio-lateral projection, of a prominence of the anterior superior corner of the calcaneum, forming a kind of beak, apparently in contact with the navicular bone.

The author emphasizes the fact that this »apophyse calcanéo-scaphoïdienne» does not justify the signification applied to it by N.-J., and he demonstrates this by ordinary photos of the foot-skeleton, one of them taken in the straight medio-lateral position, the other with the lateral border of the planta raised about 45° from the photographic plate. These pictures show that the contact between this »apophyse» and the naviculare, insisted on by N.-J., cannot be proved by radiograms taken in the first-named projection only.

ZUSAMMENFASSUNG

Unter Bezugnahme auf seine frühere Publikation: »On Coalitio calcaneo-navicularis» im Journal of Orthopedic Surgery for 1921 erörtert Verf. die Theorie betreffs der Kombination dieser anatomischen Anomalie mit Plattfusssymptomen und -deformität, die er dort aufgestellt hat und die er weiter aufrechthält. Verf. widerlegt die von NOVÉ-JOSSERAND in einem Aufsatz »Formes anatomiques du pied plat» (Revue d'orthopédie, 1923) erhobenen Einwendungen gegen seine Auffassung. Im genannten Aufsatz bezieht sich N.-J. auf zwei junge, von ihm selbst beobachtete Fälle solcher Knochenvereinigung und auf einen von BAISCH beobachteten Fall mit leichtem Plattfuss, von dem N.-J. behauptet, dass es sich um einen Fall von Knochenvereinigung gehandelt habe. Aus diesen drei Beobachtungen zieht N.-J. den Schluss, dass der in derartigen Fällen auftretende Plattfuss nur eine temporäre, durch reflektorischen Muskelspasmus erzeugte Funktionsstörung sei. Seine Diagnose der Knochenvereinigung gründet er im letzteren Falle nur auf das Aussehen einer schnabelförmigen Prominenz am vorderen, oberen Kalkaneusfortsatz auf einem in rein

medio-lateraler Projektion aufgenommenen Radiogramm, welche Prominenz offenbar mit dem Navikulare in Kontakt gestanden sei.

Verf. betont, dass die Bedeutung, welche N.-J. dieser »apophyse calcaneo-scaphoïdienne» zuschreibt, nicht gerechtfertigt ist, und demonstriert dies durch gewöhnliche Photographien des Fussskelettes, eine in rein medio-lateraler Position aufgenommene, und eine, bei welcher der laterale Rand der Planta ungefähr 45° von der photographischen Platte gehoben war. Diese Bilder zeigen, dass der Kontakt zwischen der erwähnten »Apophyse» und dem Navikulare, auf den sich N.-J. stützt, sich durch Radiogramme, die nur in der erstgenannten Position aufgenommen sind, nicht beweisen lässt.

RÉSUMÉ

L'auteur, référant au mémoire: »On Coalitio calcaneo-navicularis» publié par lui, en 1921, dans le *Journal of Orthopedic Surgery*, discute la théorie qu'il avait déjà émise dans cette première publication, et sur laquelle il insiste toujours, concernant la relation entre cette anomalie anatomique et les symptômes et la déformation du pied plat. Il réfute les objections faites à ses vues, par M. Nové-Josserand, dans un mémoire publié, en 1923, dans la *Revue d'Orthopédie;* dans lequel mémoire M. N.-J. cite deux jeunes cas de coalition de sa propre observation, et un cas de pied plat faiblement prononcé, observé par Baisch, qu'il déclare également une coalition. De ces trois observations, M. N.-J. conclut que le pied plat résultant dans les cas de ce genre n'est qu'un accident temporaire, déterminé par un spasme réflexe musculaire. Son diagnostic de coalition, dans le dernier cas, se base sur rien que l'apparence, dans un radiogramme pris dans une projection nettement médio-latérale, d'une saillie de l'angle antérieur-supérieur du calcanéum, formant une espèce de bec d'après les apparences en contact avec l'os naviculaire.

L'auteur insiste sur le fait que cette »apophyse calcanéo-scaphoïdienne» ne justifie nullement la signification que lui attribue M. Nové-Josserand. Cela, il le prouve au moyen de deux photos ordinaires de la squelette du pied: l'une prise dans la projection directement médio-latérale, l'autre avec le bord latéral de la plante soulevé à un angle d'environ 45° de la plaque photographique. Les photos montrent que le contact, sur lequel insiste M. Nové-Josserand, de cette »apophyse» avec l'os naviculare, ne peut pas être prouvé par des radiogrammes pris seulement dans la première de ces projections.

FROM THE MEDICAL CLINIC OF THE UNIVERSITY OF COPENHAGEN, DENMARK, CHIEF: PROFESSOR DR. C. LUNDSGAARD, AND THE RÖNTGENOLOGIC CLINIC OF THE RIGSHOSPITAL, COPENHAGEN, CHIEF: DR. H. PANNER

CHOLECYSTOGRAPHY

A Description of the Method of GRAHAM and COLE and a Report of our Results[1]

by

Eggert Møller and *David Ottosen*

(Tabulæ XXVII—XXVIII)

Introduction

The results derived from röntgenologic examination of the gall-bladder have been of but little diagnostic value so far. Some authors (GEORGE and LEONARD, especially) claim that on close examination of the röntgenogram a faint shadow, presumably corresponding to the gall-bladder, may frequently be observed in pathological cases; this was however refuted by others, and GRAHAM and COLE's method has enabled us to establish that, in most cases, these shadows have nothing to do with the gall-bladder. The so-called indirect signs of disease of the gall-bladder, consisting chiefly in certain alterations in the shape of the duodenum filled with a baryta-meal and in dislocations and peristaltic abnormities in the pyloric region, have likewise been established to be of little significance. Attempts have been made at producing a more distinct shadow of the gall-bladder by inflating air into the colon or into the peritoneal cavity; the latter of these methods involves a certain risk; they are both unreliable, and, at best, they yield but little information.

The Appearance of Graham and Cole's Method

There is however an entirely different mode of procedure which gave promise of far better results. We have witnessed the revolution effected in the study of the pathology of the stomach by the introduction in 1904 by RIEDER of the systematic application of the opaque meal. In addition to this the methods of pyelography, bronchography and röntgenography after injection of lipiodol or

[1] Read before the Danish Society for Internal Medicine, February 5th, 1926.

jodumbrin into the subdural space, into anal and pleural fistulæ, have been introduced more recently.

There was thus every reason to try to introduce an opaque substance also in the gall-bladder. BURCKHARDT and MÜLLER succeeded in doing so (in 1921) in a series of cases by injecting a solution of collargol directly into the gall-bladder through the abdominal wall. This method was of course too dangerous and unreliable to be applied to human beings.

In February 1924 the problem was solved in a much more elegant manner by GRAHAM and COLE of St. Louis, U. S. A.

The main principle of their method is the introduction into the organism of a substance which 1) is excreted into the bile, 2) produces a shadow in the röntgenogram and 3) is of little or no toxicity in the dosage administered.

The compound which was first recommended for this purpose by GRAHAM and COLE, after extensive animal experimentation, was calcium tetrabromphenolphthalein. It belongs to a group of chemical compounds, the derivatives of phenolphthalein, which in recent years have found extensive application also within medical science. ABEL and ROWNTREE showed in 1909 that many of these substances are excreted into the bile. This obtains especially for phenoltetrachlorphthalein which was, on this account, used for a liver function test by ROWNTREE, HURWITZ and BLOMFIELD (1913). By introducing a sulphogroup into the molecule of phenolpthalein one gets phenolsulphonphthalein, which is chiefly excreted through the kidneys, and which was therefore used by ROWNTREE and GERAGHTY (1912) in their well-known test for the secreting power of the kidneys. It has been shown (by E. MØLLER and C. LUNDSGAARD (1925)) that in the normal human organism a small quantity of this substance is also excreted through the liver. — Furthermore, CLARK has introduced the use of some derivatives of phenolphthalein as indicators in the colorimetric method for pH-determination.

GRAHAM and COLE recommended to inject the calcium tetrabromphenolphthalein intravenously and take röntgenograms at the expiration of 4, 8, 24, and 32 hours. A well-marked shadow was then obtained in the normal cases. The method is often named only GRAHAM's method or cholecystography. In some cases the injection of the dye brought about a more or less pronounced, but rapidly subsiding acute intoxication in the patient. These so-called »reactions» will be further dealt with later.

The method was soon adopted by several physicians in America and elsewhere and various modifications have been suggested. These will be mentioned each separately.

Which Substance Should be Preferred as a Contrast Agent?

As early as 1924 GRAHAM, COLE and COPHER recommended, instead of the calciumsalt of tetrabromphenolphthalein to use the sodium salt, the latter being more easily soluble and less apt to produce untoward reactions in the patients. However, reactions continued to occur, appearing in about one half of the cases and being occasionally fairly pronounced. Therefore it was a great advance when WHITAKER and MILLIKEN of Boston in January 1925 recommended the use of sodium tetraiodophenolphthalein.

These authors explain the superiority of the latter compound in the following manner. The shadow-producing property of these two substances is due only to their content of bromine and iodine, respectively. The power of iodine in this respect is proportional to that of bromine as about 1: 6, since the atomic weight of the former of the two elements is proportional to the latter as 176: 79. Besides, the higher atomic weight of the iodine has the effect that, computed in percentage, the tetraiodine has a higher concentration of iodine than the tetrabromine has of bromine (61 and 50 per cent., respectively). These two facts taken together explain that a certain dose of tetraiodine produces a shadow of almost double the intensity as that produced by the same dose of tetrabromine. The toxicity of the two substances being practically equal, it is thus a great advantage that, when using tetraiodine, it is sufficient to give doses of only half the amount as those required when using tetrabromine, i. e. 3 gm. and 5,5 gm., respectively.

In accordance with these facts, it is reported from all sides that the reactions have become of less frequent occurrence and less pronounced since the introduction of tetraiodine; and, besides, the shadows are generally more distinct.

The sodium salt of tetraiodophenolphthalein is a bluish-grey powder. It is fairly unstable, both as regards the influence of light, and in aqueous solution. In a decomposed state it is more toxic and gives less distinct shadows than otherwise.

In this connection we shall mention that, according to a communication published in May 1925, SABATINI and MILANI had for a long period used to take röntgenograms of the gall-bladder subsequent to the patients' ingestion of 15 to 20 gm. of sodium bromid, by which method they had obtained a well-defined gall-bladder shadow in 60 per cent. of all the normal persons examined. These results, however, are so inferior to those arrived at with GRAHAM

and COLE's method, that the Italian method in its present form will scarcely be of any practical value.

In what Way Should the Contrast Agent be Introduced into the Organism?

Owing to its locally irritating properties the compound cannot be administred subcutaneously or intramuscularly. Moreover, the solution has an unpleasant taste, and, introduced into the stomach, it will cause nausea and vomiting.

From these and other reasons GRAHAM and COLE concluded that the contrast agent should be administered intravenously.

It has later been proposed to give it by mouth in the form of pills or capsules, coated with some substance which was insoluble in the stomach, but should disintegrate in the intestinal canal. WHITAKER and MILLIKEN recommended for this purpose salol. MENEES and ROBINSON use gelatine capsules hardened by means of formaldehyd.

STEWART and RYAN tried keratinised pills; GRAHAM, COLE, MOORE and COPHER use phenylsalicylate; other authors have proposed stearic acid or other fats for coating. None of these procedures, however, seems to be fully satisfactory; in some cases the pills are not at all dissolved, in others only partly so, the gall-bladder shadows consequently becoming indistinct or failing to appear at all in cases in which a subsequent examination by the intravenous method revealed normal shadows.

As the absence of a gall-bladder shadow (as will be mentioned later) is an important finding which, with great probability, denotes pathological changes, it will readily be understood that, as for accuracy, the oral method of administration is somewhat inferior to the intravenous. GRAHAM, COLE and COPHER also expressly state this in their latest publication (December 1925). They report that in a large number of cases, which were examined by their method prior to operation, the röntgenologic diagnosis proved correct in 95 % of the cases, in which the substance had been given intravenously, while it was correct in 80 % only of the cases in which it had been given by mouth.

At some hospitals — as for instance at the MAYO-Clinic — administration by mouth is the routine method, but, if the result is doubtful, especially if a shadow fails to appear, the intravenous method is used as a control.

It has been attempted to render the »oral» method more reliable by giving larger doses by mouth than those applied intravenously.

However, if it happens that all the pills are completely dissolved simultaneously, the possibility of a reaction (at least of a local one) becomes greater (cf. later).

Among other attempts at avoiding the intravenous mode of administration, it may be mentioned that STEWART and RYAN in 40 cases administered the tetraiodine in a solution through a duodenal tube reaching as far as into the jejunum. Reactions occurred, however, in a number of the cases, both in the form of vomiting and diarrhoea. And besides, the introduction of a duodenal tube into the jejunum is a far more protracted and unpleasant process for the patient than an intravenous infusion. The same obtains with regard to the introduction of the substance by means of a drip-enema administered in the course of 1 1/2 to 2 hours, as suggested by STEGEMANN (Königsberg). Reactions were seen also with this procedure; their number is not stated.

In accordance with our present knowledge in regard to these points, it seems justifiable to infer that the intravenous method — in the form which will be described in the following pages — is to be preferred. If, for some reason or other, it is not advisable to use it (veins unsuited for injection), it may be attempted to administer the tetraiodine by mouth in the form of salol-coated pills of about 0,20 gm., a total dose of 4 to 6 gm. The pills should be taken in the evening, one at a time, in the course of a few hours. The following day the entire abdomen should be röntgenographed in order to ascertain that the pills have been completely dissolved; however, if the gall-bladder shadow fails to appear, one cannot draw such definite conclusions as otherwise from this finding.

It is of some interest to note that for ambulatory examination both the oral and the intravenous method have repeatedly been reported to have yielded satisfactory results.

Technique Used in This Publication

A. Preparing the Patient. (Infusion of Contrast Agent)

We have employed the intravenous method throughout, giving in all cases the same dose, namely 3 gm. of the tetraiodine compound. The intestinal canal of the patient should be thoroughly purged during the two days preceding röntgenography, as a rule with castor oil. The last meal (tea and bread) is given at 4 p. m. the day prior to examination. At 6 p. m. intravenous infusion of 3 gm.

sodium tetraiodophenolphthalein[1] dissolved in 100 gm. freshly distilled, sterile water. A fresh solution is prepared for each administration; it is filtered and sterilized by boiling in water-bath for 15 minutes. We employ for the infusion a blood-transfusion apparatus with two receptacles. A sterile physiological salt solution is poured into the one receptacle and allowed to fill all the pipes and rubber-tubes of the apparatus. The pinch-cocks are now turned off and the tetraiodine solution poured into the second receptacle. After the production of stasis a vein in the elbow-joint is punctured by means of a cannula, to which is fastened a piece of rubber-tubing about 5 cm. long. When the blood begins to flow freely, the bandage is loosened, and the point of the connecting pipe is introduced so as to fit closely into the rubber-tube of the cannula. The cock of the salt-water receptacle is turned on; when the water runs freely, it is again turned off, and the second receptacle containing the tetraiodine solution is now opened. When this receptacle is empty, both tubing, pipes, cannula, and, last not least, the vein, are thoroughly rinsed with salt water in order to entirely remove the dye-stuff solution. The tetraiodine has a tendency to stick to the walls of the rubber tube, but this may be prevented by rubbing the same while the second portion of the salt water flows in. It should take in the least 5 minutes for the tetraiodine solution to enter the vein. In the 42 infusions administered by us, the time of infusion varied from 4 to 20 minutes.

When the infusion is accomplished the patient is allowed to drink some water, but otherwise he must fast until the röntgenologic examination is completed. The examination is commenced the following morning at 9, that is 15 hours subsequent to infusion, and as a rule röntgenograms are taken 3 to 4 times in the course of the morning until about noon or a little later.

B. Röntgenological Technique

It is absolutely necessary to procure pictures which, from a technical point of view, are perfect. We have laid great stress on securing that the X-rays are projected as rectangularly as possible on the frontal plane of the patient, so that the same projection is obtained in all cases, which is of importance for comparing the individual pictures. The photographs are taken with a POTTER-BUCKY diaphragm, COOLIDGE tube, 50—60 milliampère, about 70 Kilo voltage

[1] Our preparation has been supplied by MERCK. The price is about 1 Krone (Danish) pr. gm. We keep it in a dark place in small, well-corked phials, each containing 3 gm.

and a time of exposure of 1 to 2 seconds, that is, comparatively soft tubes and a fairly short time of exposure. If the latter could be still further reduced, it would no doubt be an advantage, partly because it is often difficult for the patients to keep their breath to the extent required, and, also, on account of the peristaltic movements. It is of importance that the patient be carefully instructed beforehand.

Most of our röntgenograms were taken between 15 to 20 hours subsequent to infusion. In a few cases we have moreover taken some films at different hours, the result being that our experiences agree with those of most other observers in that the shadow is most distinct between 16 to 22 hours after infusion. Experiments have also shown (Cophier) that the iodine concentration in the gall-bladder is highest during the said period.

Physiological Basis of the Method

The contrast agent introduced into the organism will under normal conditions be excreted from the liver cells together with the bile. The latter will flow into the gall-bladder which, by continual water resorption, is able to concentrate its content up till 6,4 times (Rous und Mc Master (1921)), and does not expel it till a reflex movement from the duodenum causes the gall-bladder to contract.

The following conditions are necessary for the production of a normal gall-bladder shadow by cholecystography.

1) A sufficient quantity of the contrast-substance should be introduced into the circulation; 2) The liver-cells must be able to excrete it; 3) D. hepaticus and D. choledocus must be passable, so that there is no biliary stasis; 4) D. cysticus must be passable; 5) the cavity of the gall-bladder must be sufficiently large (i. e. a proper gall-bladder must, of course, exist, its cavity must not be reduced by shrinking, by the presence of stones, or pressure from outside to such an extent as to prevent its being sufficiently filled with the contrast substance); 6) the gall-bladder must possess the necessary power of concentrating its contents; 7) there must not be any fistular openings from the gall-bladder, for instance into the intestines or into the stomach; 8) the function of Oddi's sphincter (which is situated on D. cysticus at the juncture of the other biliary ducts) must be unimpaired; 9) no contraction of the gall-bladder must take place in the period intervening the infusion of the contrast agent and the röntgenographic exposure.

Of these conditions points 3, 4, 5, and 6 are especially impor-

tant in regard to the interpretation of pathological röntgenograms, as will be further mentioned below. Point 2 does not play any special rôle in this respect, as it seems that the power of the liver-cells to excrete the contrast-substance always in the living organism will exceed the minimum value required for shadow production. Quite differently in the case of biliary stasis, as for instance in C. pancreatis, since the unimpaired liver-cells are not able to excrete the substance against the increased pressure.

Points 1 and 9 are especially important in regard to the correct effectuation of the method. Point 1 — the dosage and introduction of the contrast agent — has been discussed in the preceding pages. We shall therefore now only mention the experiences concerning the reflexes of the gall-bladder which have been gained by means of GRAHAM and COLE's method. The most important investigations have been carried out by SOSMAN, WHITAKER und EDSON. They found that solutions containing sugar, amylum, extract of meat, or ginger, ingested by normal persons, had no influence on the gall-bladder shadow; whereas the shadow was distinctly reduced after the ingestion of solutions containing proteins, and, more especially, fats. Magnesium sulphate solution administered by way of a duodenal tube could produce some reduction of the shadow. KAZNELSON and REIMANN found that ingestion of oil had a distinct influence on the shadow. Furthermore, SOSMAN, WHITAKER and EDSON observed that even vigorous bodily exercise, especially involving the abdominal musculature, had no effect whatever on the size of the gall-bladder shadow. This is especially important for the question of the examination of out patients.

Normal and Pathological Röntgenograms

The normal gall-bladder shadow is always very distinct and sharply outlined. Its location is stated to be highly varying (LANGE), although no exact measurements in this respect have been reported. It may be roundish, pear-shaped or cylinder-shaped. A normal shadow should have an intensity above a certain minimum which the observer will learn through experience. It has been stated that on röntgenograms taken at intervals of about 1 hour, the normal shadow should always vary somewhat in shape.

LANGE found that on the individual's changing from the recumbent to the erect position the shadow is generally displaced from 4 to 10 cm. downwards. This displacement is greatest in thin and asthenic persons.

The following *pathological* findings have hitherto been described:

1) Negative stone shadows (see Fig. 8), i. e. a distinct gall-bladder shadow presenting one or more »holes», due to cholesteriden stones, since these do not produce a shadow. In order that this picture shall appear, the gall-ducts must be passable, and the gall-bladder must have retained at any rate some of its power of concentration.

2) Absence of gall-bladder shadow. The more important causes of this picture are, partly, stones which block up D. cysticus, and, partly, cholecystitis, which has caused the gall-bladder to shrink considerably or to lose its power of concentration. Experiments on animals have shown that, if the mucous membrane be removed by abrasion of the gall-bladder, the latter is deprived of its shadow-producing power in cholecystography.

3) Faint shadow. This is also attributable to reduced power of concentration. This finding should no doubt be estimated with much caution.

4) Unchanged shape in several röntgenograms taken at intervals of at least half an hour is stated to indicate a chronic fibrous cholecystitis. The value of this finding is also somewhat doubtful.

5) Disfiguration of the shadow, due to adherences. CARMAN and COUNSELLER claim that this diagnosis is as a rule not corroborated by operation.

6) Especially deep location has been considered pathological by PRIBRAM, GRUNENBERG and STRAUSZ; these authors use the term »cholecystoptosis». This point of view has not been shown to be correct.

Investigation of the Motor Function of the Gall-Bladder

By cholecystography we obtain information not only of the anatomy of the gall-bladder, but also of its physiological relations. The important rôle of the power of concentration of the gall-bladder has already been mentioned. In addition to this, it may be said that just as the introduction of the opaque meal gave the impetus to important studies in regard to peristaltics and evacuation of the stomach, the introduction of GRAHAM and COLE's method affords a possibility of studying the motor function of the gall-bladder.

So far, as mentioned above, some experiments on the gall-bladder reflexes in the normal have been reported, but no investigations of patients with diseased gall-bladders. Besides, SOSMAN, WHITAKER and EDSON experimented with subcutaneous injection of pituitrin,

adrenalin, atropin, physostigmin, or pilocarpin, without seeing any distinct or constant effect of any of these substances. On the other hand, PRIBRAM, GRUNENBERGER and STRAUSZ constantly found a reduction of the shadow after subcutaneous injection of hypophysin.

Our own investigations in this respect are as yet too few to justify further description. We shall just say that in patients with presumably healthy gall-bladders we found reduction of the shadow after ingestion of cream, buttered bread, oatmeal porridge and baryta paste, as also after subcutaneous injection of pituitrin, while hypophysin seemed to be less active. Fig. 2 shows the gall-bladder shadow of a normal, fasting patient. Fig. 3 shows the same, one hour after the patient had eaten bread and butter. These investigations will be continued.

Reactions

It has been mentioned that the introduction into the organism of the contrast agent sometimes gives rise to unfavourable reactions. These have become less frequent and, especially, less pronounced, since tetraiodine was substituted for tetrabromine. The reactions may be divided into local and general.

a) *Local reactions*

Introduction of the contrast agent into the stomach gives rise to cardialgia, nausea and vomiting. The same effect has been produced by infusion by means of a tube into the jejunum, and by administration by mouth of pills coated with some stuff or other; in some cases diarrhoea was the result. Local reaction has been noted also in the intravenous method, in the form of phlebitis, or even thrombo-phlebitis. Thus, of SCHÖNDUBE and KALK's cases, 5 out of 30 developed thrombosis. Such reactions are undoubtedly due to the fact that the solutions applied often have been of too high concentration (for instance a $12\,^1/_2$ % solution of the tetrabromine salt) and, besides, too rapid infusion and, subsequently, omission of a thorough rinsing with salt-water. In the 42 cases examined by us the tetraiodine was given intravenously in a 3 % solution, and we have seen no local reaction whatever. It is important to take care that not the least of the substance is spilled outside the vein, as it is strongly locally irritating. A number of cases have been described with painful infiltration, and even necrosis, resulting from paravenous injection. Among our cases, nothing of this kind has happened.

b) *General reactions*

These are presumably due to a toxic action upon the nervous system. The symptoms appear from a few minutes to an hour after injection, while their appearance is less regular when the contrast agent is administered by mouth, since, in that case, they will depend upon the disintegration of the pills and the resorption of the substance. In its milder form the reaction consists of malaise, headache, nausea, cardialgia, and, besides, in some cases, vomiting, diarrhoea and rise of temperature, sometimes accompanied by shivering fits. In a few cases fall in blood-pressure and collapse were likewise seen. The symptoms always completely subsided in the course of a few hours, at most.

GRAHAM, COLE and COPHER stated (in 1924) that general reaction is more common in woman than in man, which statement we can confirm. We have given tetraiodine 27 times intravenously to 22 male patients without experiencing any reaction whatever, whereas a general reaction occurred in 6 out of the 15 female patients examined by us. One of these was subjected twice to the examination, without reaction in either case (cf Table 3). We shall report the reactions of each case separately: 1) Case 19. Immediately following the infusion the patient grew a little pale and felt slightly unwell for a few minutes. 2) Case 32. Five minutes after the infusion was accomplished the patient felt unwell, with palpitation and a pressure in the head; this lasted for a quarter of an hour. 3) Case 30. One hour subsequent to infusion the patient had nausea, cardialgia and an isolated attack of vomiting; this reaction lasted an hour. 4) Case 16. An hour after infusion nausea, cardialgia and vomiting, once only. The temperature rose to 38.3° C. This lasted an hour. 5) Case 33. Had headache, cardialgia and nausea a few minutes subsequent to infusion; a few vomitings and rise of temperature to 38.7° C. This reaction lasted 1 1/2 hour. 6) Case 36. Had a shivering-fit which began 3/4 hour after infusion and lasted about an hour. The temperature rose to 39° C. Afterwards, she fell asleep and felt no discomfort later.

In none of these 6 cases was the reaction of a perilous nature. There is not in our experiences any correlation between the duration of the infusion and the occurrence of a general reaction.

Reaction is more apt to occur in elderly persons than in young people, very nervous women being notably susceptible to reaction. Otherwise, the disease of the patient seems to play no rôle in this respect, especially not a diseased state of the liver and gall-bladder.

Neither biliary cirrhosis, mechanical or functional icterus seem to dispose to general reaction (LANGE, and others). This agrees well with the fact (established by SOSMAN, WHITAKER & EDSON) that occlusion of D. choledochus in dogs gave rise only to a slight augmentation of the toxicity of the substance, while it always prevented shadow-production.

A few authors, only, have given figures denoting the frequency of reaction in their patients, and none has hitherto stated anything as regards the sex and age of their patients. The few figures at hand cannot therefore serve for a comparison between the different methods of administration (intravenous, oral, etc.) in regard to the frequency of reaction.

The reactions are *treated* symptomatically. CARMAN recommended adrenalin. PRIBRAM, GRUNENBERG & STRAUSZ advise to give a prophylactic dose of atropin subcutaneously some time previous to injection. We have done this, but, according to our present knowledge, we think that it has no appreciable effect.

It has been considered that the risk of a general reaction might be reduced by giving a dose of the contrast agent which had been calculated according to the body-weight. All our 37 patients received the same dose, a total of 3 gm., and, consequently, the leaner persons did receive the larger amount per kilogramme body-weight. It proved, however, that these were not the most disposed to get reactions, rather the contrary (cf. Table 3).

Owing to the occurrence of general reactions it was thought necessary, in the initial phase of cholecystography, to dictate various counter-indications for its application, such as heart-disease, hypertension, phthisis, diabetes, etc. Most of these may, however, now be disregarded.

SARALEGUI has used the intravenous method in cases with hypertension, valvular disease, and »arythmic pulse», without seeing any reaction. SOSMAN, WHITAKER & EDSON declare (December 1925) that they do not know of any condition that should counter-indicate examination by this method. This is perhaps going a little too far. In our opinion, one should be wary in the use of cholecystography in patients who in a marked degree suffer from diseases of the circulation, more especially in cases of elderly, nervous women.

Practical Results of Cholecystography

All who have tried the method and communicated their results agree as to its great importance. It is as yet too soon to give a final judgment as regards its diagnostic value in each of the various

diseases in which it is applied. The material at present available is, however, sufficiently extensive for an orientation. The cases in which the röntgenological examination has been followed by operation, so that the gall-bladder has been either inspected and palpated or examined after removal, are of course of special value. Pathological examination naturally gives the most reliable information.

Such a material of operated cases has hitherto been supplied by 5 different clinics, 2 American, 2 English and 1 French. We have correlated the results of these investigations in Table 1 and given an abstracted survey in Table 2.

Table 1

cases reported up to the present in which the gall-bladder has been examined first by cholecystography and afterwards by operation.

Author	Mode of administration of contrast agent	Normal shadow			Absence of shadow			Negative stone shadows		
		Normal gall-bladder	Cholecystitis, but no stones	Stones	Normal gall-bladder	Cholecystitis, but no stones	Stones	Normal gall-bladder	Cholecystitis, but no stones	Stones
Gosman, Whitaker & Edson	intravenously	4	1	—	—	4	10	—	—	17
Carman	intravenously	—	2	2	—	3	21	—	—	3
»	per os	16	5	5	3	16	29	—	—	15
Cohen & Roberts	intravenously	2	—	—	1	—	4	—	—	—
Wilkie & Illingworth . .	intravenously	21	—	3	2	—	—	—	—	4
Gosset & Loewy	in most cases intravenously	21	—	—	—	1	9	1	—	—
		64	**8**	**10**	**6**	**24**	**73**	**1**	—	**39**

Table 2

Abstracted survey of the cases specified in table I.

		Findings at operation			Total
		Normal gall-bladder	Cholecystitis but no stones	Stones	
Röntgenological findings	Normal shadow	64	8	10	82
	Absence of shadow	6	24	73	103
	Negative stone shadows . . .	1	—	39	40
		71	**32**	**122**	**225**

Much discretion must be exercised in estimating the figures of these tables, because the material is highly selected in so far as operation has been indicated in all the cases. Accordingly, among other things, the figures for normal gall-bladders with normal shadow show comparatively much lower values than those of the other groups.

It appears from Table 2 that a normal gall-bladder will give a normal shadow in 91 per cent., in the least, of all cases (and probably in a still higher percentage), while, on the other hand, it is seen that cases *may* occur in which no shadow appears on the röntgenogram, although a subsequent operation did not reveal anything abnormal.

Furthermore, a gall-bladder affected with cholecystitis, but not with stones, will in some cases produce a normal shadow, while in most cases no shadow appears.

Table 2 shows that of 122 operated cases of gall-stone, the 10 only gave normal shadow, while in the remainder the shadow was absent, or negative stone-shadows appeared.

In one case the röntgenogram was interpreted as »negative stone-shadows», but no stones were found on operation. This erroneous interpretation was due to airbubbles in the colon, situated in front of the gall-bladder. This was of course a mistake due to want of practice in estimating the pictures, which is always avoidable in the future. Except in this one case, the finding of »negative stone-shadows» has always proved to be an unfailing sign of gall-stones. It should be added that the figures in Tables 1 and 2 do not include cases in which gall-stones could be detected in the usual röntgenograms taken without previous introduction of a contrast agent (positive stone-shadows).

Again, Table 2 shows the frequent absence of a gall-bladder shadow. This emphasizes the importance of interpreting this negative finding correctly, and points to the great advantage of using the intravenous method which, at present, is the most reliable in this respect.

Our Own Investigations

These cannot be compared with those mentioned above. Hitherto only two of our patients were operated. As a beginning our efforts have been aimed at 1) developing our technique, and 2) at forming an opinion of the relations in normal persons, based on our own observations. Moreover, 3) we have during the last months examined all the patients admitted to Department A, who showed

any signs of affection of the biliary ducts, and 3 cases from Department D.

ad 1) — Our *technique* has been described in the preceding pages. Both the infusion and the röntgenograms were in all cases successful from a technical standpoint.

ad 2) — *Examination of normal persons.* We have carried out 25 examinations of 22 patients, whose symptoms did not indicate any affection of the biliary ducts, neither judging from the anamnesis, nor from the result of an objective examination especially directed toward this point and therefore also including testing for urobilinuria and occult icterus. Most of these patients suffered from chronic obstipation, colitis or (certain) ulcus ventriculi. In Table 3 these patients are specified as cases 1 to 22.

In two of these cases, Nrs. 21 and 22, cholecystography gave no shadow of the gall-bladder. Case 21 was subjected to two examinations, with the same result. In this patient the formation of adherences around the ulcer may possibly have prevented the gall-bladder's being filled with the contrast-substance (cf. case 26). Case 22 offers no clue in this respect.

The remaining 20 cases all showed a distinct gall-bladder shadow. Two of them were subjected to two examinations, both agreeing in all points. Figures 1, 2, 3, 4 and 5 represent five normal pictures. The main impression derived from the pictures of these 20 normal gall-bladders is the striking individual difference as far as location is concerned.

By some *measurements* we have attempted to supply a quantitative determination of the size and location of the gall-bladder shadow. Attention should be drawn to the fact that as the measurements have been performed on the röntgenograms, the results give only indirect information of the actual anatomical relations in the patient. The mutual relations on the picture will always depend on projection. Although we suppose that the influence of projection is small (provided the necessary precautions are taken during röntgenography), it will always in the individual case be a factor of unknown quantity.

Still, we think that these measurements may be of interest as an attempt at determining the range of variation of the size and location of the shadows on the röntgen-film produced by the contrast agent in normal gall-bladders under the given conditions.

The results are given in Table 3. We have used only the pictures which were taken 15 hours after the infusion. As far as the 20 normal individuals (cases 1—20) are concerned the table shows the following:

Table 3

results of own investigations

Case Nr.	Initials	Sex	Age	Height (cm)	Body weight (kg)	Diagnosis prior to cholecystography	Finding in cholecystography 15 hours subsequent to infusion	Gall-bladder shadow: Size: Length	Size: Breadth	Location: at the level of	Location: distance from middle line to: apex of gall-bladder (a)	Location: distance from middle line to: lateral abdominal wall (b)	Location: a/b	Reactions
1	K.O.	M	20	165	52	Neurasthenia	normal shadow	5.0	2.7	1.-2.l.v.*)	6.0	15.0	0.40	0
2	A.A.	M	27	169	73	Obstip. chr.	» »	5.5	2.5	1.l.v.	6.0	17.5	0.34	0
3	L.J.	M	28	167	73	Ulc. ventriculi	» »	4.5	2.5	12.d.v.+)	11.0	18.0	0.61	0
4	N.N.	M	29	165	54	Obstr. chr.	» »	5.0	2.0	2.l.v.	15.0	16.0	0.94	0
5	J.N.	M	29	178	69	Ulc. ventr. (juxtap.?)	» »	9.0	3.2	2.-3.l.v.	6.5	16.0	0.41	0
6	H.J.	M	30	171	76	Colitis chr.	» »	8.0	3.5	1.-2.l.v.	4.7	14.5	0.32	0
7	M.S.	M	32	177	60	Ulc. ventriculi	» »	6.0	3.5	3.-4.l.v.	7.0	15.0	0.47	0
»	»	»	»	»	»	» »	» »	6.5	2.0	3.-4.d.v.	7.0	15.5	0.45	0
8	C.J.	M	32	172	51	Ulc. ventr. (juxtap.?)	» »	5.0	1.5	2.-3.l.v.	?	?	?	0
9	K.B.	M	33	174	57	Ulc. ventr. (juxtap.?)	» »	6.5	2.5	4.l.v.	6.0	15.5	0.39	0
10	H.J.	M	37	176	66	Colitis chr.	» »	5.0	3.0	1.-2.l.v.	?	?	?	0
11	C.P.	M	49	165	65	Myopathiæ variæ	» »	3.5	3.5	1.l.v.	10.0	16.0	0.63	0
12	H.H.	M	50	170	56	Obstip. chr.	» »	8.0	4.0	3.-4.l.v.	10.0	16.0	0.63	0
»	»	»	»	»	»	» »	» »	?	?	3.-4.l.v.	?	?	?	0
13	V.P.	M	51	—	75	Ulc. duodeni	» »	6.0	3.0	1.l.v.	11.0	17.0	0.65	0
14	S.P.	M	52	175	60	Obstip. chr.	» »	6.0	3.0	3.l.v.	5.5	14.5	0.38	0
15	L.C.	F	27	151	39	Bronchitidis seq.	» »	7.5	3.0	2.-3.l.v.	4.0	12.5	0.32	0
16	O.C.	F	33	154	54	Obstip. chr.	» »	6.5	3.5	12.d.v.-1.l.v.	7.0	15.0	0.47	+
17	F.P.	F	39	166	79	Obstip. chr.	» »	6.0	3.5	1.-2.l.v.	7.5	18.5	0.41	0
18	K.N.	F	42	152	49	Colitis chr.	» »	6.5	3.0	2.-3.l.v.	7.5	15.0	0.50	0
19	I.H.	F	45	—	—	Ulc. corporis ventr.	» »	6.0	3.0	1.-2.l.v.	8.5	17.0	0.50	+
20	A.A.	F	50	153	68	Arthroitis genus	» »	7.5	3.6	12.d.v.-1.l.v.	8.0	17.0	0.47	0
21	C.T.	M	23	179	68	Ulc. ventriculi	no shadow	—	—	—	—	—	—	0
»	»	»	»	»	—	» »	» »	—	—	—	—	—	—	0
22	S.N.	M	53	172	77	Neurasthenia	» »	—	—	—	—	—	—	0
23	N.N.	M	52	168	59	Cholelithiasis	Positive stone shadow, no gall-bladder shadow	—	—	—	—	—	—	0
24	J.L.	M	54	—	—	Cholelithiasis		—	—	—	—	—	—	0
»	»	»	»	—	—	»	»	—	—	—	—	—	—	0
25	B.L.	F	38	—	—	Cholelithiasis	Positive stone shadow, faint gall-bladder shadow	—	—	—	—	—	—	0
26	E.N.	M	26	163	55	Ulc. duodeni	No gall-bladder shadow	—	—	—	—	—	—	0
»	»	»	»	»	»	» »	»	—	—	—	—	—	—	0
27	H.P.	M	40	168	58	Cholangitidis seq.	normal shadow	5.5	2.0	2.-3.l.v.	3.0	15.0	0.20	0
28	E.N.	F	28	163	49	Obs. p. cholelithiasis	» »	5.5	2.3	12.d.v.-1.l.v.	6.5	15.0	0.43	0
29	S.S.	F	35	161	57	Obs. p. cholelithiasis	» »	5.5	2.5	1.-2.l.v.	5.5	14.5	0.38	0
»	»	»	»	»	»	» »	» »	5.0	2.0	1.-2.l.v.	5.0	14.5	0.34	0
30	A.U.	F	37	157	70	Obs. p. cholelithiasis	» »	6.0	3.5	12.d.v.-1.l.v.	7.5	17.0	0.44	+
31	I.S.	F	41	166	74	Obs. p. cholelithiasis	» »	6.0	3.0	1.-2.l.v.	9.0	16.0	0.56	0
32	A.H.	F	56	—	—	Obs. p. cholelithiasis	» »	8.5	3.7	2.-3.l.v.	8.5	16.0	0.53	+
33	V.S.	F	59	156	60	Obs. p. cholelithiasis	» »	?	?	2.-3.l.v.	?	?	?	+
34	O.O.	M	55	169	83	Obs. p. cholelithiasis	No gall-bladder shadow	—	—	—	—	—	—	0
35	H.N.	F	52	150	62	Obs. p. cholelithiasis	»	—	—	—	—	—	—	0
36	E.H.	F	51	165	69	Obs. p. cholelithiasis	Negative stone shadows	9.5	4.0	1.-2.l.v.	13.5	15.5	0.89	+

*) l.v. — lumbar vertebra.
+) d. v. — dorsal vertebra.

a) *Location.* The position of the shadow in the vertical direction is seen to vary between the level of the 12th dorsal and the 4th lumbar vertebra. For determination of the position in the lateral direction the apex of the base of the shadow has been used. This is the point of the whole shadow which can be most easily marked out. The distance of this point from the middle line is found to be from 32 to 94 per cent. of the distance from the middle line to the lateral abdominal wall.

b) *Size.* The length of the shadow varies between 3.5 and 9.0 cm. and the breadth 1.5 and 4.0 cm. The latter measure is the more accurate of the two, as it will very often be difficult to decide exactly at which point the gall-bladder shadow ends in the upward direction.

c) The *intensity* of the shadow shows some variation in the individual cases.

— In some cases the shadow had the form of a hook. Whether this be due to a projection phenomenon, a physiological variety, or to some pathological condition (adherences?) we cannot as yet determine.

ad 3) *Patients with symptoms indicating diseases of the gall-bladder.* This group comprises 14 patients. The total number of examinations is 17 (cf. Table 3).

a) 3 cases (Nrs. 23, 24 and 25) in which simple röntgenological examination had revealed gall-stones. At cholecystography no gall-bladder shadow appeared in the two first cases. In Case 24 there was, however, a faint shadow surrounding a dense positive shadow of an isolated roundish stone. This patient was operated (at Department C) and a large pigmented calculus corresponding to the picture was found, and, besides, a few lesser similar stones.

b) Case 26. Three months prior to the examination ulcus duodeni with marked adherences around the gall-bladder had been observed in operation. Cholecystography produced no shadow (2 examinations). In this case it is reasonable to assume that by pressure on the gall-bladder or on D. cysticus the adherences have prevented a sufficient filling of the gall-bladder with the contrast agent.

c) The remaining 10 patients, Cases 27—36, were clinically very similar to each other, all presenting more or less marked symptoms of a chronic affection of the bile-ducts, without it being possible in any of the cases with certainty to recognize or to exclude such a disease.

In 7 of these cases cholecystography showed normal gall-bladder shadow. It is true that this finding does not absolutely exclude a mild cholecystitis or a small calculus in the gall-bladder; still, it

makes the presence of either of these diseases very doubtful. — At any rate, the result of the Röntgen examination shows that all the bile-ducts are freely passable and that the power of the gall-bladder to concentrate its contents is preserved. And, this information is of greater import than to determine, whether or not there is an isolated calculus in the gall-bladder.

Writers on this subject have hitherto chiefly been concerned with cases, in which such pathological conditions could be demonstrated by means of GRAHAM & COLE's method, that surgical operation could afterwards be resorted to. Still, it should be emphasized that cholecystography is also of great value in cases like those of our last seven patients, where, by means of this method, we could almost with certainty exclude essential alterations in the bile-ducts. In a number of such cases of uncertain diagnosis, in which medical treatment is of no avail, the surgeon will often be obliged to proceed to operation, an operation which will in many cases be chiefly explorative. Where cholecystography shows a normal shadow, such an operation may in some cases be avoided.

In 2 of the 10 cases mentioned under c) cholecystography gave no gall-bladder shadow. Accordingly, the diagnosis is considered to be cholecystitis or cholelithiasis. In neither of these cases was there as yet indication for operation.

The last case (Nr. 36) showed negative stone-shadows. It presented a peculiar gall-bladder shadow (cf. Fig. 8) reaching very far laterally, being S-shaped and presenting numerous small »holes». The patient was operated on, and the shape and location of the gall-bladder was found to correspond exactly to the röntgenogram; moreover, it contained 54 cholesterin-stones, ranging in size from that of a pea to a bean. Fig. 9 shows the gall-bladder after removal in a dissected state. We need not further emphasize the value of GRAHAM & COLE's method in such a case.

Our own results have quite come up to the expectations with which we began to employ this method. At Department A of the Rigshospital the method will enter as a permanent factor in the routine examination of patients presenting symptoms of biliary affections.

Our knowledge of the symptomatology of these affections is but slight, being primarily based on the study of the more severe and advanced cases. For instance, it is a well-known fact that in most of the cases, in which gall-stones are found at the post mortem examination, the diagnosis has not been made in vivo. Bearing in mind the valuable clinical results derived from the introduction of the opaque meal in the study of the diseases of the stomach, we

are justified in expecting great results from the work which, in the years to come, will certainly be conducted all over the world for the elucidation of the diseases of the gall-bladder and the bile-ducts by means of GRAHAM & COLE's method.

SUMMARY

1) A description is given of GRAHAM & COLE's method for röntgenological visualisation of the gall-bladder; of the development of the method up to the present time, and of the most important results it has yielded.

2) Our technique for intravenous infusion of the contrast agent and for röntgenography is described.

3) The method described has been applied in 36 cases, with 42 examinations altogether.

4) Local reaction did not occur in any case.

5) Mild or moderate general reaction occurred in 6 cases.

6) A description is given of the röntgenograms of 22 cases which must anticipatively be considered as free from diseases of the gall-bladder and the bile-ducts. An attempt is made at giving exact measurements for the size and localisation of these gall-bladder shadows.

7) A report is given of the results arrived at in 14 cases in which previous routine examination was more or less in favour of diseases of the bile-ducts.

ZUSAMMENFASSUNG

1) Die Verfasser geben eine Beschreibung der Methode von GRAHAM und COLE zur röntgenologischen Sichtbarmachung der Gallenblase und berichten über die Entwicklung der Methode bis zum gegenwärtigen Zeitpunkt sowie über die wichtigsten Resultate, die sie bisher gegeben hat.

2) Sie beschreiben ihre Technik für die intravenöse Infusion des Kontrastmittels und für die Röntgenphotographie.

3) Die beschriebene Methode wurde bei 36 Fällen mit zusammen 42 Untersuchungen angewendet.

4) Eine lokale Reaktion trat in keinem Falle auf.

5) Leichte oder mässige Allgemein-Reaktion kam bei 6 Fällen vor.

6) Beschreibung der Röntgenogramme von 22 Fällen, bei welchen man voraussetzen konnte, dass sie von Erkrankungen der Gallenblase und Gallenwege frei seien. Die Verfasser versuchen, exakte Masse für die Grösse und Lokalisation dieser Gallenblasenschatten zu geben.

7) Bericht über die erhaltenen Resultate in 14 Fällen, bei welchen eine frühere Untersuchung in der bisher üblichen Art mehr oder weniger für Erkrankung der Gallenwege gesprochen hatte.

RÉSUMÉ

1) Description de la méthode de GRAHAM & COLE pour la visualisation radiologique de la vésicule biliaire; du développement actuel de cette méthode et des résultats qu'elle a donnés.

2) Technique de l'injection intra-veineuse de l'agent de contraste de la radiographie.

3) La méthode décrite a été appliquée dans 36 cas, avec un ensemble de 42 examens.

4) On n'a jamais observé de réaction locale.

5) Réaction générale faible ou modérée dans 6 cas.

6) Description du röntgenogramme de 22 cas préalablement considérés comme exempts de toute affection de la vésicule ou des voies biliaires. L'auteur essaie de donner une mensuration exacte de l'étendue et de la localisation de ces lésions vésiculaires.

7) Compte-rendu des résultats obtenus dans 14 cas, dans lesquels un examen antérieur par les méthodes anciennes avait plus ou moins fait soupçonner des lésions des voies biliaires.

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FROM THE ROENTGEN DEP'T OF THE FREDERIKSBERG HOSPITAL, COPENHAGEN
CHIEF: DR. P. FLEMMING MØLLER, M. D.

MEASUREMENTS BY MEANS OF THE SIEMENS' DOSIMETER AND THE SABOURAUD PASTILLE, AND BY THE FORMER IN CONNECTION WITH A TUBE OF SABOURAUD PAPER

by

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With the increasing development of the Roentgen technique, and with the constantly greater perfection of the apparatus used in the Roentgen therapy, it also becomes increasingly necessary for the Roentgen operator to know, to within a degree of exactitude as minute as in any way possible, the precise dosage which he is employing in the treatment of his patients. If the enormous advances made, of recent years, both in Roentgen technique and in Roentgen physics, cannot fail to be a source of the utmost satisfaction and joy to every roentgenologist, it is nevertheless an unquestionable fact that those same advances have enormously increased the dangers of practising the Roentgen therapy; and in this connection no circumstance, perhaps, is more noticeable or momentous than the fact that, with the considerably shorter time in which it is now possible to administer the dose desired, a hardly unavoidable error in dosing is apt to be immensely more fatal.

When, in a roentgen clinic, a modern, stronger apparatus has been substituted for an older one less powerful, it very often happens that, during the first time of using the new apparatus, the operators are surprised by observing a stronger effect of the irradiation, far in excess of their intention or desire. The reason of this has been the subject of a great many different explanations in the different cases; but, as I shall explain in greater detail later on, a number of circumstances seem to indicate that, of the same identical dose, the biological effect is stronger if the dose is given in short time with greater intensity, than if it is given in longer time with less intensity. I shall not, in this place, discuss the question as to which

of those two methods of dosing is preferable, therapeutically; I only wish to point at once to the fact that here, already, we have a new difficulty and a fresh element of danger intruding itself.

When a roentgenologist has been working with a certain apparatus for a considerable length of time he will, little by little, have acquired a fund of purely empirical experience, thanks to which the cases will be relatively few in which he risks to be suddenly confronted with inexplicable, over-intense irradiation effects or — much less — actual roentgen burns, regardless of whether he uses one method of treatment or another, or this or that method for measuring the intensity of the radiation. But the whole problem of dosage, and the intricacy of the as yet incompletely elucidated question concerning the exact biological effects of the roentgen rays, makes it easy to understand that those unwished-for happenings *may* occur, without it being possible to point to any actual error, or lack of judgment, on the part of the operator.

The fitting of an institution with one of those highly effective, modern roentgen apparatus makes it imperatively incumbent on the roentgenologists put in charge of that apparatus to familiarise themselves as thoroughly as possible with every detail of its characteristics, and to follow, to the very utmost, the demands for the greatest possible exactness of dosage. This is the case in which we have recently been placed, at the Frederiksbergs Hospital, by the installation there of a modern, highly perfectioned, so-called »stabilivolt» apparatus. I may say, at once, that we should hardly have been equal to the difficult situation thus created if we had not, at the same time, possessed, in the Siemens' Dosimeter, a measuring device most excellently fulfilling all the physical requirements that could justly be made to an apparatus of that kind. I venture to believe that some statements concerning the measurements which it has been necessary for us to make may be of interest. In my opinion, they furnish a useful aid to the better understanding of a good many questions of more general concern, connected with the roentgen treatment; though, of course, I am entirely aware that no necessarily valid conclusions can be drawn, nor any direct applications made, either from them or from the other experiences made by us, to conditions of irradiation as practised elsewhere and with the use of different types of apparatus.

The conditions for our measuring would seem to have been, in every way, perfectly good. If such measurements are to be even approximately accurate, the first requirement must necessarily be that the radiation to be measured is as constant as possible, and that the possibilities are present for obtaining an extensive range of

variation in the emission of the rays. At the same time, the measuring apparatus must be one which, in itself, is proof against too gross errors; it must be trustworthy and must register finely, though not too finely.

All those conditions have been fulfilled by our »stabilivolt» apparatus and our Siemens' dosimeter. Furthermore, the protective lead-box has been a means of avoiding disturbing radiations from other parts of the tube.

It is a well-known fact that the curve of the radiation produced by such a high-tension, unidirectional apparatus — if it is not exactly a straight line — is at least extremely flat, and that the emission of the rays is by no means as oscillating as with the sinusoidal current from the ordinary transformer, or as shown by the jagged curve in the case of the induction-coil. This is of the very greatest importance as regards the measuring; because, with any of the two last-named transforming devices, the needle of the measuring apparatus will to a certain extent follow the variations of the current, with the result that the measurements will be less exact. I have also tried to do some measurements using our photographic apparatus; but although the latter could be used for the purpose I found it impossible to obtain results with them anywhere nearly as fine as with the »stabilivolt» apparatus. With the last-named it is possible, by regulating at the same time the tension and the strength of the current, to keep those two factors absolutely constant, even for hours, if one so wishes.

Here, as always, a point of difficulty is to know the exact voltage with which one is working in each single instance, so as to be able to conclude, in that given case, what is the smallest wave-length, or at what wave-length the greatest intensity is to be found. Our so-called »kilovoltmeter» is, of course, a simple voltmeter, on the primary side, serving to calculate the secondary tension on the basis of the transformer's potential of conversion. But inasmuch as the result of that calculation only holds good as long as the machine is run *without* the roentgen tube, it must necessarily be taken for granted that the voltages with which we are actually working are lower — though we cannot say *how much* lower — than those indicated by the »kilovoltmeter». At the Siemens Works they are at present engaged in establishing curves that shall show the actual voltage by measurements over a spark-gap between two spheres, each of 25 cm diameter; but we have not yet received those curves. With unidirectional apparatus, as we all know, the tension at the top of the curve is equal to the mean tension.

We have not had the opportunity to make any spectrometric

measurements for the ascertainment of wave-lengths; but as far as I am able to understand, that sort of measurements are not always equally informing, because it is difficult to determine exactly the point of greatest intensity of the radiation (λ max.). It is true that the shortest wave-length (λ_0) can be found more or less exactly; but that can be done in another and easier way, provided the tension is known, simply by applying the equation: $\lambda_0 = \frac{12.35}{V}$.

I would therefore like it to be understood that when, in the following, I shall speak of the various voltages, I shall mean the figures indicated by our kilovoltmeter; though, as a matter of fact, I am aware that the actual tension is somewhat lower. Even with strong filtration, the roentgen radiation is, of course, far from homogeneous, but will extend over several octaves. Still, the point of maximum intensity is always to be found relatively close to the shortest wave-length, and increasingly so the stronger the filtration. A look at the following Table (1) will show where, about, the shortest wave-lengths are found, under the different voltages with which we are working; the value of λ_0 being worked out, on the basis of the tension, by means of the formula just quoted.

Table 1. $\lambda_0 = 12.35$.

K. V.	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
λ i Å. U. .	1.24	0.62	0.41	0.31	0.25	0.21	0.18	0.15	0.14	0.12	0.11	0.10	0.095	0.089	0.082	0.077	0.072	0.068	0.065	0.062
				↓				↓												
				Ba 0.33				Pt 0.15												

The lowest voltage at which the apparatus can be worked with at all is about 60—70 kilovolt, which corresponds to a wave-length of, respectively, 0.21 and 0.18 ÅU (Ångström Units). The highest tension which we have ever employed was of 180 kilovolts, corresponding to 0.068 ÅU. We usually operate with a current of 6 m. a.; but we have done measurements with currents of from 5 to 8 m. a. When using copper filter we generally work with a tension of 180 kilovolts; when using aluminum filter we work with only 150 kilovolts.

It is necessary that I should speak very briefly also of the principle of the Siemens' Dosimeter. The irradiation of the ionisation chamber produces a very feeble ionisation current, of 10^{-10} ampères' strength, passing from the electrode inside the chamber, through a very heavy resistance, to the ground, and thence back to the wall

of the chamber. The difference in tension between the clamps of the resistance acts as a »grid» tension on the anode current in a circuit through an intensifying-tube. It is this 1 000 times stronger anode current which is registered by the galvanometer of the dosimeter. Inasmuch as, under certain circumstances, the anode current is a function of the »grid» resistance, which, in its turn, is in proportion to the ionisation current, we thus obtain a measurement for the latter. But the determining factor in connection with the measuring apparatus is the proportion existing between the anode current and the tension on the clamps of the resistance. That proportion is different for each single intensifying-tube and is expressed in a following curve.

The figures read directly on the galvanometer have, thus, no value in themselves; in each instance it is the corresponding voltage-figure on the curve that has to be looked for. We have been so fortunate as to be able to use the same intensifying-tube during all our measurings, and it has not undergone any noticeable changes during all that time.

The reading of the curve is accurate as regards the two first figures, but the third figure cannot be read quite exactly. In order to avoid possible errors in reading from the curve, I have substituted for the latter a table, thus making sure of always getting identical voltage-figures for identical values. When it is a question of calculating penetration-percentages and the like, even small mistakes in reading may very well result in an error of as much as 2 per cent.

Inasmuch as the tension is proportional to the ionisation current, I have, in the comparative measurements, used simply the voltage figures. In the daily work, the measuring of the ionisation current was easily made by using a nomogram; but for the more exact measurements I deemed that method unsuitable, as it is apt to give rise to further errors of reading.

The weak point of the dosimeter undoubtedly lies in its large resistances. There are three of those resistances, and they are subject to variations both with temperature and with change of pressure, to such an extent that they are never the same from one day to another. The errors that may result on that account are by no means negligeable, but it has been tried to counteract them by making so-called: standard measurements. These consist in a daily determination of the resistance by application of Ohm's law, substituting for the ionisation chamber a chamber containing oxide of uranium, which produces an invariable ionisation current — in our case of 2.24×10^{-10} ampères. But not even that means of standardization has sufficed

to eliminate the source of error with our apparatus, which comes from the fact that the ratio between the different resistances is not constant from one day to another. Thus between R_1 and R_2, the ratio can vary from 3 to 3.3; and R_3 is still more inconstant. The strongest ionisation currents are measured with R_1, and those measurements are undoubtedly the most exact ones. R_2 is a little less exact; and R_3 I do not very much care to use at all. It is best, if in any way possible, to make all the measurements with the same resistance; but that is not always feasible.

In order to get perfectly accurate measurements it is necessary, not only that the measuring apparatus itself should be kept constant, but also that the greatest pains be taken to maintain the most perfect possible accuracy of tension, strength of current, and of the roentgen apparatus as well. But when all those points are carefully looked after, one may also have the pleasure of finding that precisely identical results of the measurements will be obtained time and again, even for hours at the time, and almost identical results from one day to another.

How great has been the mean average error in our measurements, it would be rather difficult to say. The maximum error is undoubtedly the one which results when, in calculating the percentage, it is a question of dividing by one another two values of measurement, both of which are already in themselves more or less slightly inaccurate. In such cases the error may probably run as high as to about 5 per cent; in a few, very unsuccessful instances it has been as high as 10 per cent.

An enormous advantage of the Siemens' dosimeter consists in the fact that it measures the instantaneous value. With many other iontometers, where the discharge of an electroscope has to take place for several seconds, there will undoubtedly be far greater chances of errors resulting, both from unevenness in the speed of the discharge and from the impossibility of keeping the roentgen tube absolutely constant for the requisite length of time. Nor would it have been possible with any other apparatus to encompass so great a number of measurements as we have been making.

As regards that first and exceedingly important question: as to whether the roentgen tube always produces the same intensity of radiation to the second, we must — as far as our apparatus is concerned — answer: yes!, as it may be seen from the series of measurements made during twelve days (see *Table 2*). It will be observed that the variation from the mean figure has been only 5 per cent to either side; and of some of those hundredth-parts it may

Table 2.

	180 KV.		6 M. A.		40 cm.			½ Cu + 1 Al				
Days	1	2	3	4	5	6	7	8	9	10	11	12
Io . .	5.6×10^{-10}	5.4	5.4	6	5.7	5.8	5.8	5.9	5	5.6	5.8	5.4

Average: 5.7×10^{-10} Oscillation ca. 5 %.

probably even be supposed that they were due to some error in measuring.

During the various interruptions we have had with the roentgen apparatus, and during the frequent changing of tubes, the measuring apparatus has been absolutely indispensable to us. By its aid we were always able, under the altered conditions, to find the proper exposure-times easily and quickly. On one occasion, when we had put in a fresh roentgen tube, we discovered, by means of the measuring apparatus, that the intensity was slightly less; and, right enough, we found that the tube had been placed about 0.5 cm too high up in the protection-box; something which, otherwise, we probably should not have noticed at all.

Of course, the measured ionisation current does not correspond to the full intensity of the roentgen radiation per second, but represents only the fraction of the radiation which is absorbed in the ionisation chamber, and which is converted there into the electron-radiation that produces the ionisation-current. What percentage is thus absorbed depends, as we know, on a number of circumstances: the size of the chamber, the material of which it is made, etc. Consequently, it is not an absolute measure for the dosage; but one may, of course, have one's measuring apparatus standarised to roentgen-units by comparing it with a large condensed-air ionto-quantimeter; an instrument from which all those sources of error are eliminated which, as we know, attach to the usual small ionisation-chambers. But even a standarisation of the iontometer, as here indicated, to a measure so absolutely and always reproducible as the roentgen-unit, will only apply to one given, definite tension, filter and composition of rays. Consequently, the dosimeter makes it possible to reproduce the identical dose, *but only under identical conditions.*

Physically, the dose is the product of milliampèreage and time of exposure. Therefore, if this product is known, the time required for giving the same dose can always easily be found even under different

conditions of radiation-intensity per second. Furthermore, it seems to be so fortunate that, under conditions of high tension and strong filtration, the quality of the radiation remains more or less constant even if the tension varies as much as 20 or 30 kilovolts; whence it results that the same rules for dosage can be adhered to even if the strength of the current is, for instance, increased from 150 to 180 kilovolts. But it is expressly admitted by BEHNKEN and GROSSMANN, and by others as well, that this proportionality does not altogether hold good in case of weaker filtration (through aluminum) and does not exist at all for unfiltered radiation. It is therefore impossible to deny that, as far as determining the dose that can be supported by the skin under qualitatively various conditions of radiation is concerned, the ionisation measurements are by no means as practically helpful as it might be wished.

To find out how much of a dose the skin can stand there is, therefore, no other way open for us than to proceed by experiment: through observation of the manner and degree in which a series of patients react to a variety of dosage; or, in other words, we must determine *empirically* what to consider as the skin erythema dose.

As I shall speak of again in a few moments, it unfortunately depends somewhat on individual conditions what, in the case of different patients, constitutes the skin-erythema dose; and also the conception of what should be considered a standard S. E. D. seems to vary a great deal, as proved by the well-known comparative measurements made by MARTIUS.

In Denmark, the roentgenologists have, up to the present, deemed it inadvisable to abandon that simple and — in spite of the many more or less well-founded objections that can be made against it — truly ingenuous measuring method: the Sabouraud pastille. This attitude is all the more reasonable because — by a wonderful coincidence — that purely physical method really seems, in the case of medium-hard, non-filtered radiation, to be exactly proportional to the skin dose.

If many roentgenologists elsewhere have found it advisable to abandon the Sabouraud pastille for other methods of measuring it is, of course, among other things, because that proportionality ceases to hold good in the case of filtered radiation, owing to the considerably greater doses which the skin is able to support of the latter. The resulting error we Danish roentgenologists generally compensate by increasing the »Sabouraud dose» progressively as thicker and stronger filters are used. Thus, as far as I know, it is the usually accepted practice among Danish operators to consider 4 S. the dose that can be given when a 5 mm. aluminum filter is used, and 5 S.,

or perhaps more, to be the proper dose with a 0.5 mm. zinc or copper filter; the pastille being placed at one half of the focus-distance.

At the Frederiksberg Hospital, when we began using our new stabilivolt apparatus, we wished to bring those old experiences of ours to bear on our work with the latter; but inasmuch as it was not possible, here, to place the tablet at one half of the focus-distance, we were obliged to place it at 23 cm., on the irradiation-tube, and divide by 4, in order to get the time for 1 S. But it very soon turned out that when we used the number of »Sabouraud»s which we believed proper according to the tablet, the reactions produced were decidedly in excess of what we aimed at. If our old experiences no longer held good it therefore must be admitted that there were various features about the usual Sabouraud measurements which made them more or less irrational. When the Sabouraud pastille is measured at half the focus-distance it is only primary radiation that is measured, while neither the secondary radiation nor the size of the field are taken into account.

In order to get a numerical estimate of the rôle played by those influences, we measured, by means of the iontometer, the increase of the surface-intensity on the skin, on a water-surface and on a block of paraffin, respectively; noting, at the same time, how much more rapidly the tinting of the Sabouraud pastille took place under those circumstances, when using a field of 100 cm^2. As it might be expected, the measurement with dosimeter on paraffin showed an increase of about 33 per cent for the dose, per second; measurement with the Sabouraud pastille showed a corresponding shortening in time of one third; and similar measurement on water an increase of about 20 per cent — there being undoubtedly a quite appreciable difference between the capacity of paraffin and water for producing secondary radiation. For skin, the increase lay about midway between the two.

If it is a question of determining the penetrating dose, the importance of the secondary radiation becomes decidedly greater.

Of the bearing which the size of the field has on the magnitude of the surface-dose, an idea may be had by looking at the curve no. 1 of which it will be noticed that from a markedly steep beginning it gradually becomes quite flat. To this it should be remarked, however, that as regards the very small fields the curve conveys a wrong impression, in so far as the measured dose becomes smaller than it really is, owing to the fact that not all of the ionisation chamber is irradiated. In the case of the penetrating dose the size of the field plays a much greater rôle, as we shall see in the following.

It is worth noticing that already quite empirically we are wont to take the conditions here mentioned into account. Thus, for in-

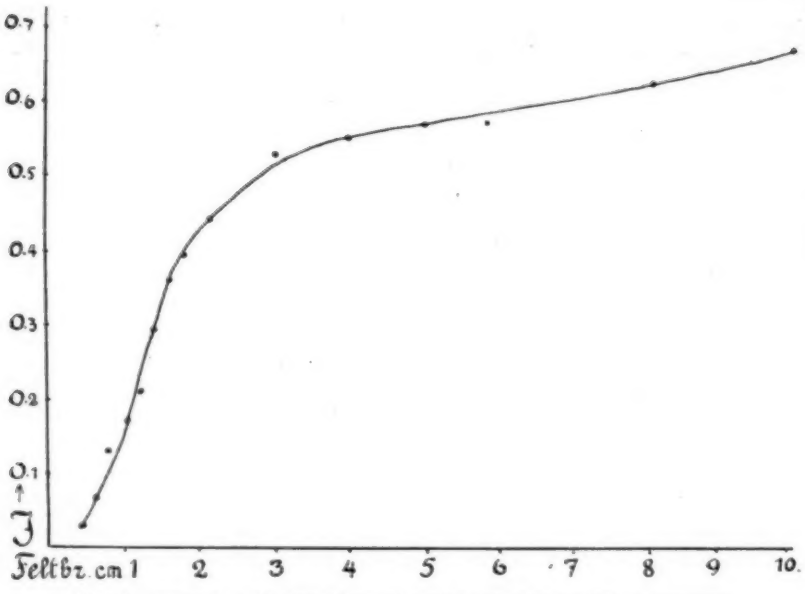

Fig. 1. Curve 1. The dependence of surface intensity on field of exposure.

stance, we can irradiate a small field, such as a wart or an epithelioma, with 2 S. unfiltered or with 3 S. — and perhaps even more — through 0.5 mm. Al. The curve shows that between two fields of, respectively, 100 cm^2 and 1 cm^2 the difference in intensity is about 3 times higher in the case of the former.

On the basis of our experiences with the too strong reactions we reduced the exposure time in the manner that we counted with the length of time required for the blackening of a Sabouraud pastille placed on top of the paraffin block; for safety's sake both at a distance of 23, 30 and 40 cm. In so doing, we made the further, unexpected discovery that the time required for the blackening at a distance of 23 cm. was somewhat less than one third of the time required to produce the same result from a distance of 40 cm. In other words: the times registered did not tally with the law according to which »the effect of the rays varies inversely with the square of the distance». For instance, our times were: on paraffin at 23 cm., 16 minutes; at 40 cm., 70 minutes. Measured in the air, the blackening of the pastille, at 23 cm., took 23 minutes. But at the same time it was plain to us that the blackening at the shorter distance took place too rapidly; and we thought that this might pos-

sibly be due to heat-effect. It might be imagined that scattering from the filter, at the short distance, was the cause of the remarkably rapid blackening, owing to the filter being placed rather close to the pastille, — in the case of our apparatus as much as 3 cm. away, however. By measuring the time required for the blackening of the pastille from a distance of 30 cm., both with the filter lying immediately over the pastille and when it was placed at a considerable distance from the latter, we found that the time was exactly the same under both conditions. Consequently, scattering from the filter must be eliminated as a possible cause of the curious circumstance observed. And besides, the time required for the blackening of the pastille at 23 cm. distance continues to be too short, regardless of whether filtered or unfiltered radiation is used.

At the same time we observed another curious thing — which was probably merely accidental, however; namely, that the inverse square law suddenly held good when the pastille was measured at 23 cm. distance in the air and at 40 cm. on the paraffin block. Thus, the time for 1 S. in the air, at a distance of 23 cm., became the right one anyhow; and the two errors simply offset one another. Since then we have used as working basis for our calculations, right along, the time required for blackening the pastille, placed on the paraffin block, at a distance of 40 cm.

In order to understand what is to follow it will be necessary, then, to remember that by 1 S. — »one Sabouraud» — we understand the time required for blackening the pastille when the latter is placed at one half of the focus-distance. The protective box in which the tube is placed makes it impossible to measure this time directly, but we have calculated it on the basis of the time which it takes to blacken the pastille at a distance of 40 cm., measured at the tension used in connection with the different filters. We first calculate the true time for a distance of 23 cm. — which is one third of the 40 cm. distance — and then, from that, the time for 1 S., which is one fourth of the latter. It is very important to note — because this is a point as to which a mistake might otherwise easily be made — that the time required for blackening the pastille at 23 cm. (respectively: the 3 times longer period required for blackening it at a distance of 40 cm.) is *4 S.*, but that this is not the same as 1 erythema dose (1 S. E. D.); because, with non-filtered radiation, this time — of 4 S. — is equal to 4 S. E. D., while, with filtration through copper, it represents only about 1 S. E. D.

As a matter of experiment we decided to give 4 S. through 0.5 mm. Cu, or 3 S. through 5 Al, or 1 S. without filter; and we called those doses 1 S. E. D. We did this merely tentatively, in order to

avoid over-dosing; but the reactions which we obtained were almost exactly what we expected and desired. If our doses are considerably lower than those which we had been able to give formerly, with the »Symmetry» apparatus, and lower than what I believe is given elsewhere, it must probably be ascribed to particular features connected partly with our new apparatus itself, partly with the manner of dosing and the time, as I shall speak of more fully later on.

I may say, right here, that when we increased the tension from 150 to 180 kilovolts the times obtained, respectively, by measurement with the iontometer and the Sabouraud pastille, corresponded — with the reservations already made — surprisingly well, both in the case of filtered and unfiltered radiation. From this it would seem that, with high voltages, the same proportionality in regard to the skin-reaction, which we have already seen exists in the case of the ionisation chamber, is found also between the skin-reaction and the Sabouraud pastille, and between the two measuring devices themselves. That the altered voltage does not disturb that proportionality is due to the fact that the quality of the radiation does not undergo any essential change. This is a feature which our measurements evidently explain, as I shall show in a moment.

In the case of an apparatus so highly effective as the »stabilivolt», it is of the greatest interest to be able to form an idea as to the various directions in which it offers advantages not possessed by the apparatus hitherto employed. It is therefore natural to examine what proportion the intensity of the roentgen-radiation has to the strength of the current and to the tension.

As regards the strength of the current, it can be varied within certain limits without injuring the tube, thus enabling us to operate the tube with a current varying from 5 to 8 milliampères. We have also examined what advantages may be had from increasing the current. This is, of course, a question which has been investigated and settled long ago; the law of the matter being that: »all other conditions remaining equal, the intensity increases proportionally with the strength of the current». This is by no means absolutely true, however; especially in the case of transformers having an open iron core the strength of the current is by no means independent of the tension. Still, with our apparatus the law seems to hold good. For a rise in current from 6 milliampères to 8, the increase in intensity should be 33 per cent; and by measuring under the various tensions we have found that, as a matter of fact, the actual performance corresponds exactly with those figures. In this connection it is worth remembering that the quality of the radiation is not influenced by variations in the strength of the current.

If the strength of the current is maintained constant while, at the same time, the tension is increased, it will be seen that the intensity increases following a much steeper curve. ULREY has shown that the increase of the roentgen-ray energy, per second, is proportional to the square of the tension; while WEEKS, by measurements from

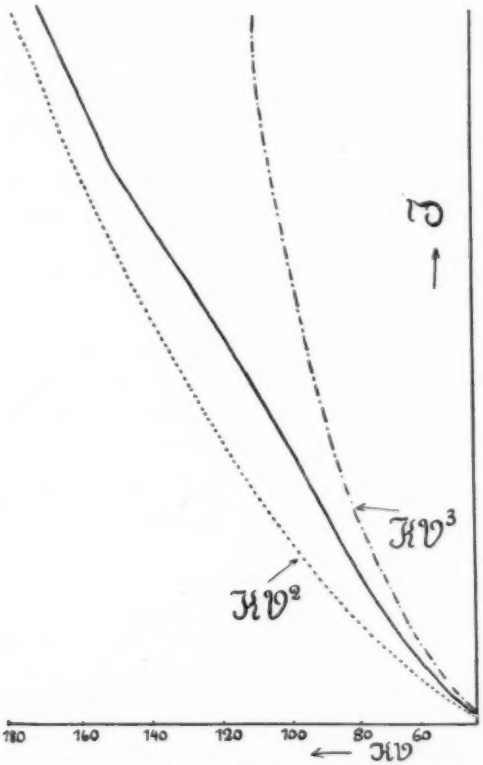

Fig. 2. The dependence of surface intensity on tension. It is seen to increase after a straight course, lying between the square and the cube of the tension.

28 to 54 kilovolts, has shown that the increase is proportional to the cube of the tension.

We have measured the intensity for tensions ranging from about 50 or 60 kilovolts up to 180, and we likewise found the result to present a very steep curve, which — curiously enough — lay almost midway between the curve which would represent the square of the tension and the one which would correspond with the cube of

the tension perhaps somewhat closer to the first-named of the two (Fig. 2).

From all this it will be seen that the exposure-time can be shortened not a little by increasing the strength of the current. But a great deal more is gained by using the high tensions. I shall give an example of this: With a tension of 180 kilovolts we can, according to our present calculations, give 1 S. E. D. — at a distance of 40 cm., through 0.5 mm. Cu — in 48 minutes. If, now, we measure the intensity of the radiation at 100 kilovolts, we find it to be seven times less than at 180 kilovolts. Consequently, the time required for giving the same dose, under the lower tension, would be 336 minutes, or five hours and a half; which sufficiently shows the enormous saving effected by working with the higher tension.

It is, of course, a generally accepted fact that the hardness of the rays increases with added tension, whereby again the penetrating dose is increased. It is therefore of the greatest importance to find out the exact proportion in which increased tension affects the penetrating power of the radiation — especially as it is hardly for purposes of surface-therapeutical work that it would be desired to install one of these large apparatus. The question here alluded to has been dealt with by GROSSMANN in his excellent work: »Physikalische und Technische Grundlage der Röntgen-Therapie», which contains curves showing that from about 120 kilovolts to 200 kilovolts the penetration increases, at most, 5 per cent. And GROSSMANN concludes that it will be impractical to attempt any increase of the tension beyond 200 to 220 kilovolts.

At the time when our measurements were made I did not know this work of GROSSMANN's, and I was both very surprised and somewhat apprehensive as to the correctness of my measurements when I found that the penetration-percentage, whether measured on paraffin- or water-phantom, by tensions of above 100 or 120 kilovolts increased so very slightly that it was difficult to prove the increase by any of our instruments. It should prove instructive, and a source of satisfaction to those operators who do not possess the new, powerful apparatus, that, as far as the penetrative effect is concerned, the same results can be obtained with tensions much lower than what we are using (Fig. 3).

It will be necessary here to put in a few words on the subject of absorption and scattering. The radiation which passes through a substance without losing any of its force cannot have any effect on that substance. Only the radiation which is absorbed is effective (MARAGLIANO). This is true both as regards physical effects and in relation to the organism; and the estimate of the penetrating dose

obtained by measuring the degree of intensity remaining after the rays have passed a certain thickness of some substance must necessarily seem more or less slightly irrational. It is true that, in reality, the effect depends on how much of this radiation is absorbed at the point desired; but, on the other hand, the radiation must, of course, be so penetrating that as much of it as possible reaches all the way down to the spot which it is wished to treat. Those two demands seem to be contradictory. As regards the deep treatment, it has so far been commonly agreed to aim at fulfilling the second

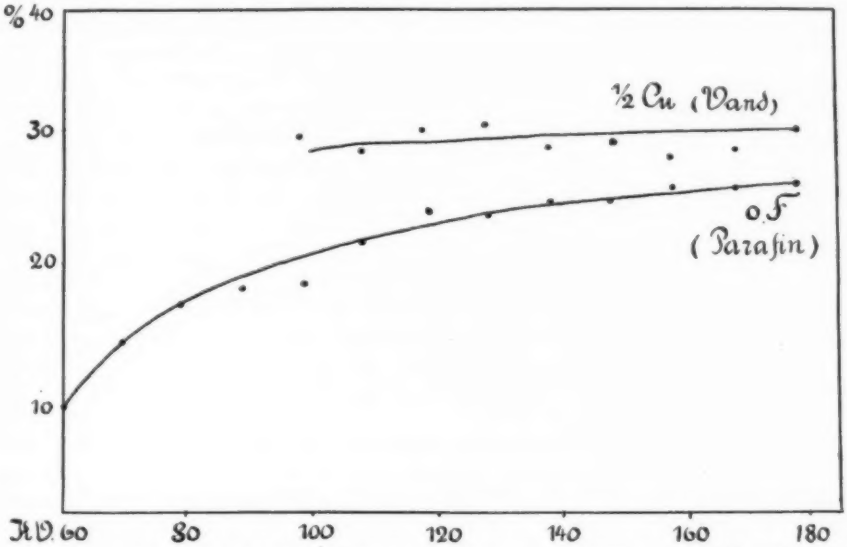

Fig. 3. The dependence of depth-percentage on the tension. At a tension above 100—120 K.W. the depth-percentage only rises slightly with increasing tension. — The upper curve, Cu-filtered radiations in water-phantom, lower curve, non-filtered radiations in paraffin-phantom.

of the two requirements, in order to obtain as uniform and homogeneous an irradiation throughout the diseased tissue as possible. For the present this is probably the only possible way. As regards the surface treatment the matter is different; there it seems to me absolutely irrational to employ too hard a radiation, when the object is to get as much as possible of the radiation absorbed in the outermost layers of the tissue. For that purpose the unfiltered radiation would certainly seem to be the best, with use of relatively low tensions; that is, tensions of less than 100 kilovolts — according to the curve, which I have just mentioned, showing the penetration's dependency on the tension. It must be remembered that the harder

radiation gives, at the same time, a considerable deep dose, and that at high tensions even unfiltered radiation has a by no means inconsiderable deep-effect, because the difference between the filtered and the unfiltered radiation to a great extent consists in the fact that, in the case of the former, the long-wave end of the spectrum is cut off, while λ_0 remains the same.

When we speak of »absorption» we can mean two different things: either — in the narrowest sense of the word — that the entire sum of radiation energy is taken up by the atom, or else that it is caught up by a free electron. It is the latter process which produces the scattered roentgen-radiation. In the following we will understand by »absorption» that part of the radiation which is held back by some one definite unit of substance, regardless of whether this »holding back» is due to absorption in the concrete sense or to scattering. In other words, we shall use the word »absorption» to denote the »weakening» of the radiation. This weakening, which occurs whenever the radiation passes through some substance or other, is due to three different factors: distance, absorption (in the concrete sense of that word) and conversion into scattered radiation. The degree of the weakening depends partly on the quality of the radiation itself, partly on the character of the substance through which the passage takes place. It can therefore best be expressed by the proportion between the coefficient of the weakening and the specific gravity; this proportion being known as the so-called »coefficient of total weakening» $(\frac{\mu}{\varrho})$ and being, in its turn, made up of the »coefficient of total absorption» $(\frac{\alpha}{\varrho})$ and the »coefficient of total scattering» $(\frac{\sigma}{\varrho})$. The value of $\frac{\alpha}{\varrho}$ depends on the wave-length, being equal to a constant λ^3. $\frac{\sigma}{\varrho}$, on the other hand, is more or less independent of the wave-length; its value, for substances of low atomic weight, being about 0.18.

From this it will be understood that the harder the radiation, the slighter does the absorption become in proportion to the scattering; and at a radiation of less than 0.2 λ — that is, a radiation harder than 60 kilovolts — it is almost the scattering alone that plays a part as regards the weakening. This, then, applies in equal measure to the human body, to water and to the paraffin block; and is probably the explanation of the fact that the penetration does not become greater with increased hardness of the radiation.

In the case of substances higher up in the periodic system the scattering becomes almost minimal in proportion to the absorption; at the same time as the selective absorption and the characteristic radiation of those substances themselves become features of account, resulting in the well-known curve of absorption with its abrupt drop in absorption from a harder to a softer radiation. It is on account of this difference in the proportion of weakening, between substances of higher and lower atomic density — and particularly on account of the suddenness with which the absorption becomes greater in the case of the former — that measuring instruments made of those heavier substances have been deemed unsuitable for work involving comparison with the organism. Thus, as we all know, the Sabouraud pastille has been the subject of considerable criticism.

In an excellent paper, published in the ACTA RADIOLOGICA (vol. IV, fasc. 1), ARNTZEN and KREBS have shown that with medium strong filtration, through 5 mm. Al, the Sabouraud pastille is in proportion neither to the biological effect nor to the ionisation effect as measured on the iontometer; the coloration of the pastille taking place to quickly. They state the time it took to administer 1 S. with, respectively, 0 filter, 5 mm. Al, and 0.5 mm. Cu + 1 mm. Al to have been, in the same order: 4, 8 and 24 minutes; while the discharging-times for the iontometer, under the same conditions, were, respectively: 13, 42 and 75 seconds. If there had been the right proportionality between the two methods of measuring, the time for administering 1 S. through 5 mm. Al should have been 13 minutes. They then measured the time it took to blacken the Sabouraud pastille with various thicknesses of filter interposed, all the way from 0 to 12 mm. Al, and found a downward break in the curve corresponding to 5 mm. Al. A repetition of the same experiment, but with the use of the iontometer, gave a comparatively straight line for the curve; still, also here, with a very slight downward break at the same point (5 mm. Al). ARNTZEN will explain this as being due to the added radiation brought to bear on the Sabouraud pastille from its own characteristic radiation, which should make itself felt at the precise filtration: 5 mm. Al. He used for his experiments a Müller's water-cooled tube in connection with a symmetry induction coil. By means of the spectrometer he found his shortest wavelength, for the primary rays, to be 0.12 Å. U., corresponding to 100 kilovolts. In order to prove that the too rapid blackening of the pastille at 5 mm. Al was really due to the characteristic radiation of the latter, he employed the truly ingenuous device of enclosing the ionisation chamber in a piece of the pastille paper and then repeating the measurements from 0 to 12 mm. Al. Also in that case

the resulting curve showed a downward break at about the same point as before, but not nearly as pronounced as in the case of the measurements with the pastille.

These investigations of ARNTZEN's were partly what induced me, first to revolve the whole problem theoretically, and then to attempt an examination of the various features connected with the Sabouraud pastille in the matter of absorption, in order to find out whether the objections raised to it are really so well-founded and of such importance as to justify us in discarding it altogether. And, more particularly, I wished to find out whether the same features which ARNTZEN disclosed would be found again when the pastille was used in connection with our apparatus and with the use of the much higher tensions employed by us. At the same time I am perfectly aware that my measurements cannot be directly compared with those made by ARNTZEN, for the simple reason that the whole disposition and technique of his experiments and mine must necessarily be in many ways dissimilar, owing to the different apparatus used, etc. Thus, for instance, ARNTZEN used a water-cooled tube, while I have been working with a Coolidge tube.

First of all, we took the times required for blackening the pastille under conditions of different tension; this being done — for the sake of economising time — at the shortest possible distance. The results were as follows:

Tension	*0 filter*	*5 mm. Al*	*0.5 mm. Cu*
180 kilovolts	2 1/2 min.	6 min.	14 min.
150 —	4 —	9 —	24 —

Especially the last line of figures tallies surprisingly well with the times registered by ARNTZEN; cu. taking about 2 1/2 times as long time as 5 mm. Al.

Next, we made a tube of the pastille paper, closed at one end with a piece of the same material. The opening at the other end, and the size of the tube itself, were such that it fitted snugly over the ionisation chamber, covering the latter completely, while at the same time it could be easily put on and removed as occasion required. To this contrivance we fitted a wooden handle, so as to avoid getting our hands within the cone of the rays while using it in the course of our measurements. The tube was made of one thickness only, of the pastille paper (fig. 4). When we covered the ionisation chamber with this tube, after first having measured the intensity of the radiation at the moment, we found, on measuring again, that the intensity decreased considerably: from 5 to over 60 per cent according to

the conditions surrounding the experiment. This can mean only one thing — namely, that the Sabouraud pastille has weakened the radiation in a corresponding degree; or — if we wish to use that expression — that it has »absorbed» the radiation.

It is undoubtedly of importance that the pastille paper should surround the ionisation chamber entirely, in order that the latter may catch up — besides the supposedly existing characteristic radiation — any existing scattered radiation as well. If we surround the chamber with two, or more, layers of the paper, the intensity of the radiation will, of course, become even less; but at the same time the conditions as to absorption in the pastille itself will become less easy to understand.

Fig. 4. Capsule of Sabouraud paper, capable of completely enclosing the ionisation chamber, for measuring the absorption in the Sab. paper (consists of one layer sab-paper, both walls and binding).

The weakening of the radiation is the same regardless of whether the barium-platin-cyanide side of the paper is toward the ionisation chamber or away from it. In all the measurements of which we shall speak here, we proceeded in the following manner:

1) The intensity of the radiation (I_0) was measured by means of Siemens' dosimeter. 2) The pastille-paper tube was fitted onto the ionisation chamber, and the new, lesser intensity (I_1) was measured. The difference between the two measurements found ($I_0 — I_1 = I_{0-1}$) must have been retained, caught up in the pastille-paper tube. What we always have wished to know — and have figured out — is: how large a percentage of one radiation or the other is becoming »absorbed» by the pastille; or, in other words, the value of the formula: $\frac{I_0 — I_1}{I_0} = I_{abs.}$

In the manner here indicated, we have made numerous series of measurements, at widely differing tensions (70, 100, 150, 180 kilovolts), trying to establish the degree of absorption for the different radiations, from unfiltered to 8 mm. Al, 0.5 and 1 mm. Cu. For practical reasons it was impossible to do any measuring with aluminum filters thicker than those named.

It will be noticed that the curves of absorption-percentages rise somewhat steeply from 0 to 1 Al, while from 1 to 8 Al the rise is more even. At certain points it happens that two values come a little closer to one another than desirable; but this we have seen to occur also, to at least the same extent — if not more pronounced — in the case of all our penetration measurements; and I am there-

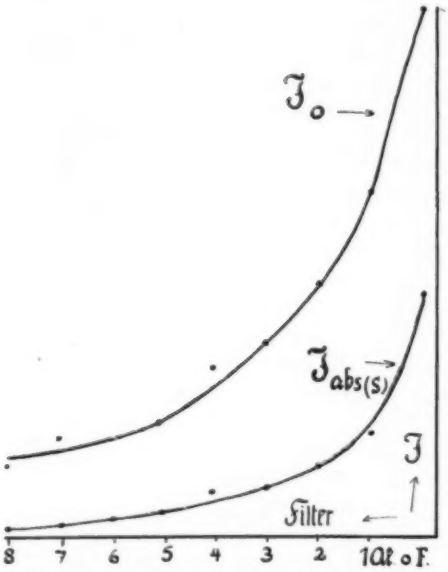

Fig. 5. The absorption by the Sab. paper in the capsule dependent upon the filtration, for comparison, uppermost the dependence of surface intensity (I_{abs} S) on the filter at the same tension (150 KW) (non-filtered to 8 mm Al).

fore convinced that such minimal variations are merely due to the fact that there are limits to the fineness of the measuring method. From 8 Al to 0.5 Cu there is another accentuation of the curves; but if we figure 0.5 Cu to be about equivalent to 10 or 11 Al there will come no sharp bend in the curve at all. *According to my measurements* — and this repeats itself at all the different tensions — there, consequently, *does not seem to be any sudden jump in the absorption anywhere.*

In the same manner — at increasing tensions of from 50 to 180 kilovolts — we have measured the percentages of absorption with unfiltered radiation, and we found that in the beginning the absorption decreased strongly, while subsequently it did so more and more evenly. At 50 to 60 kilovolts it was about 60 per cent; at 180 kilovolts only a little more than half as much — 33 or 34 per cent.

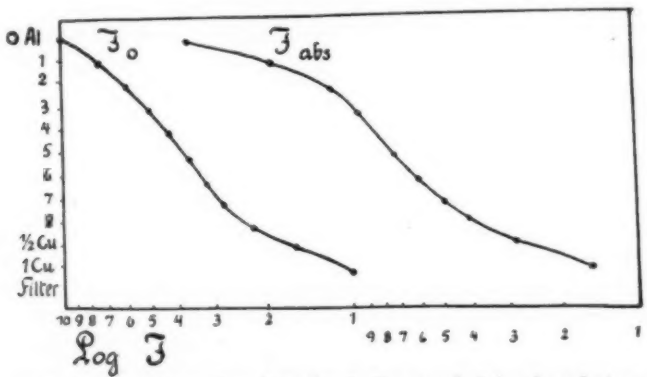

Fig. 6. Also shows the absorption in the capsule being dependent on the filter (non-filtered to 1 Cu + 1 Al): for comparison the dependence of surface intensity on filtering (Curve plotted in log units).

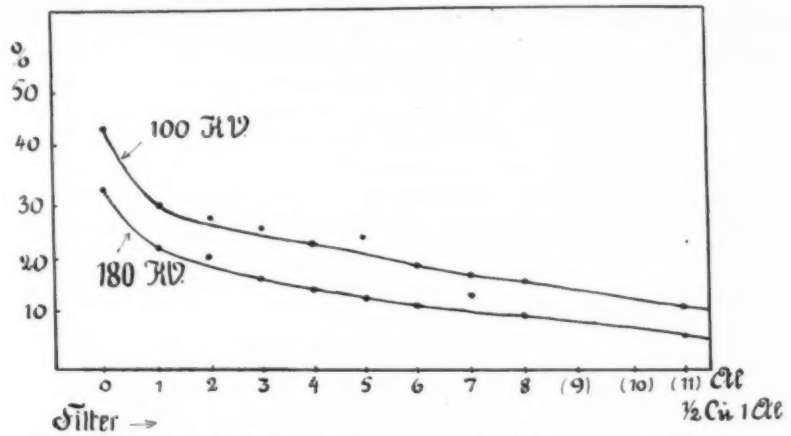

Fig. 7. Absorption of radiations by the Sab. paper in the capsule in percentage of surface intensity at 100 and 180 KW. The curve shows there is no »sudden jump» in the absorption.

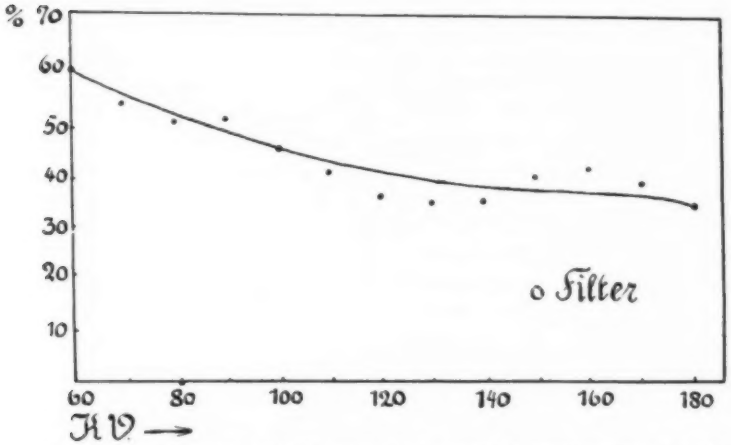

Fig. 8. The absorption by Sab. paper in the capsule dependent upon the tension in percentage of surface intensity (the curve is very flat from about 120 KW; compare Fig. 3).

The conclusions to which I have come on the basis of those observations, and of the physical facts, are as follows:

The Sabouraud pastille consists of platinum and barium in fairly even parts. As it will be seen from the table, the characteristic radiation of barium lies at λ 0.33 Å. U., which corresponds to a

voltage of less than 40 kilovolts. The characteristic radiation of platinum lies at 0.15 Å. U., corresponding to about 70 kilovolts.

As I mentioned in the beginning, heterogeneous radiation consists of many octaves; but the greatest intensity lies rather close to λ_0; which means, again, that for the purposes of any sort of therapeutic radiation we must have λ max. well on the short-wave side of the characteristic radiation of barium. As regards the platinum, the matter is more doubtful; but I do not believe that, with our apparatus, we should be able to produce a radiation of which the shortest wave-length would be found to the right of the point where the jump in absorption takes place in the case of platinum. This is as much as to say that, theoretically, the characteristic radiation of the pastille is an influence having a certain bearing on the results of our measurements; but, at the same time, the point of greatest intensity will hardly come to lie so close to the jumps in absorption that the characteristic radiation necessarily needs become proportionally strong enough to play any practical rôle, or even to be measurable at all except, perhaps, by means of specially fine instruments.

As already mentioned, the chief trouble with all the various systems and instruments for measuring the roentgen radiation comes from the fact that the proportionality between dosimeter dose and erythema dose which one may finally have succeeded in establishing for a certain quality of radiation, ceases to exist the moment that quality changes. It is not surprising, however, that this should be so, when it is remembered that in the one case it is a matter of the biological effects of the radiation, and in the other case of physical or chemical effects, and that the organism and a chemical or physical measuring device are two so essentially different objects in every way. To keep clear of the great problem as to *how* the biological effect of the roentgen rays is produced I shall, in the following, take it for granted — as, indeed, a great many writers do — that the biological effect, on the skin, for instance, remains the same regardless of the greater or lesser softness or hardness of the rays, provided only the dose is the same. By the dose we will understand the total quantity of radiation absorbed. This quantity equals the product of: 1) the intensity per second of the radiation on the skin; 2) the percentage of the intensity which is absorbed in the skin; and 3) the duration of the exposure; or, expressed as a formula:

$$D = I_1 \times I_{abs.\,1} \times T_1$$

The same dose can also be given with a different quality of radiation; that is, a different absorption and a different exposure-time; which can be expressed in this formula:

$$D = I_2 \times I_{abs.\ 2} \times T_2$$

The meaning of this can be best shown by an example. Let us take, first, a case where a dose of 100 is obtained with a radiation intensity of 100, of which 20 per cent is absorbed; the exposure-time being 5 minutes. Substituting these values in the formula we get:

$$100 = 100 \times \frac{20}{100} \times 5$$

We next chose a much harder radiation. The intensity, consequently, becomes much feebler; let us say, of 20. The tension and the strength of current we suppose unchanged. Inasmuch as the rays are harder, the absorption in the skin will be considerably less; let us put it at 5 per cent. A simple calculation will show that in this case the exposure-time, in order to obtain the same dose as before (100) must be 20 times as long, or 100 minutes. I repeat that this enormous increase in the exposure-time when a change is made from soft — unfiltered or slightly filtered — radiation to a harder, strongly filtered one is due to the two factors: 1) *weakening of the surface-intensity*, 2) *lessening of the absorption*. What I have said here can be expressed in an equation, in a form which will enable us to find the exposure-time proper to the altered conditions of radiation, if the former exposure-time, the intensity of radiation, and the ratio of absorption under both conditions of radiation are known. The formula is thus:

$$T_2 = T_1 \times \frac{I_{abs.\ 1}}{I_{abs.\ 2}} \times \frac{I_1}{I_2}$$

T_1 is the exposure-time in the first case; T_2 the exposure-time with the harder radiation. $I_{abs.\ 1}$ and $I_{abs.\ 2}$ are the percentages of the surface-intensity that are absorbed by the skin in the first and second case, respectively. I_1 and I_2 represent the intensity of radiation on the surface of the skin in each of the two cases, respectively.

It is evident that a great deal would be gained if our measuring apparatus were of a kind that would enable us to determine the absorption and the surface-intensity, independently of one another, by means of values proportional to each of the two factors. Unfortunately they are not. All the apparatus and devices so far

imagined measure those two factors together. When the proportionality between the skin and a measuring apparatus ceases, owing to altered conditions of radiation, it is impossible to see *where* the reason lies. Perhaps it is the absorption in the apparatus that now follows a curve entirely different from that of the absorption in the skin; perhaps there are unknown factors that influence more or less strongly the dose-recording of the apparatus without at the same time disturbing that of the skin. As regards those two methods: the Sabouraud pastille and the Dosimeter, there is no doubt, of course, but that the latter is by far the most precise of the two; sensitive as it is to every change in the intensity of the radiation. The usefulness of the iontometer is greatest in connection with the hard radiation. The ionisation, too, is dependent on the quality of the rays; but it seems that this process follows other laws than those which govern the absorption in the skin.

The change produced in the tint of the Sabouraud pastille is the result of a chemical process — evaporation — which likewise must depend in a greater or lesser degree on the intensity of the radiation, and on the absorption as well. As GREBE points out, it must be supposed that a portion of the absorbed radiation is transformed into chemical energy, the remainder into heat; and that with a different radiation, giving a different ratio of absorption, the portion transformed into chemical energy becomes quite another; and that this may probably be the reason why the blackening of the pastille no longer becomes proportional to the absorption. Besides this, there are undoubtedly many other factors, not necessarily directly connected either with the absorption or with the intensity of the radiation — such as heat, for instance — which, all of them, contribute to bring about the change in the colour of the pastille. There may, thus, be many reasons why the pastille does not follow the erythema dose under hard irradiation. Precisely the circumstance that under those conditions it is much more sensitive than the skin would indicate the presence of some additional factors which had no influence on the latter while, still, playing a rôle as regards the pastille. But here it lies near to think that the pastille's capacity for absorbing radiation may nevertheless, after all, be more or less parallel to that of the skin.

Now, as I described on a foregoing page, we have an extremely simple and rapid method by which it is possible to measure the absorption in a tube of platino-cyanide of barium, at any given radiation, by covering the ionisation chamber with the tube and then measuring the decrease in intensity by means of the dosimeter. The method proves the pastille to be highly sensitive to changes in the

quality of radiation. Thus, for a 100 kilovolts' radiation the percentage of absorption, when no filter is used, is 60 per cent.; if a 0.5 mm Cu + 1 mm Al filter is interposed, it drops to 10 per cent. If a proportionality really exists between the capacity for absorption possessed, respectively, by the skin and the pastille, we have, consequently, found the value of one of the factors needed in order to enable us to solve our equation. The value of the other factor — the intensity — is furnished us by the dosimeter, though only approximately: as this, too, depends to a certain extent — as I have repeatedly pointed out — on the quality of the radiation.

Theoretically there are, thus, a great many reasons to believe that the Sabouraud pastille and the Dosimeter, combined, might constitute a serviceable method for determining at least the approximately correct exposure-time — for 1 S. E. D., for instance — under varying conditions of radiation; the combination of the two methods eliminating to a certain extent the deficiencies attaching to each of them when used separately.

As regards the practical test of whether this combined method can be used, for instance, to determine the erythema dose we have, of course, as a basis of comparison only the exposure-times for the S. E. D. which we have determined empirically at the Clinic. It will be remembered that our whole system of dosage there rests, from the very beginning, on Sabouraud measurements, and that we had decided to count 4 Sabouraud with 0.5 mm Cu = 1 S. E. D.; and 3 Sabouraud with 5 mm Al also = 1 S. E. D. And it will be remembered that we found the skin reaction to those doses to be, on the whole, exactly what we desired.

On the basis of those dosage estimates I have tried the equation under various conditions of measurement, and it has invariably proved to hold good. It is my intention to test the method on a larger material; the present should therefore be regarded only as a *preliminary communication*. The following are a couple of examples to show the results arrived at:

With a current of 150 kilovolts the blackening of the pastille, at a distance of 23 cm, takes:

with no filter	4	minutes,
» 5 mm Al filter	8—9	» ,
» 0.5 mm Cu »	24	» ,

These times correspond to 4 S.

4 S. without filter = 4 S. E. D.; consequently: 1 S. E. D. = 1 minute.
4 S. through 5 mm Al = $1\frac{1}{3}$ S. E. D.; » 1 S. E. D. = $6-6\frac{3}{4}$ min.
4 S. » 0.5 » Cu = 1 S. E. D.; » 1 S. E. D. = 24 min.

Taking the unfiltered radiation for a starting point we get:

T_0 is 1 minute. $I_{abs.\ 0} = 39.8\ \%$; $I_0 = 0.98$
$I_{abs.\ 5\ Al} = 16.6\ \%$; $I_{5\ Al} = 0.37$
$I_{abs.\ 0{\cdot}5\ Cu} = 9.5\ \%$; $I_{0{\cdot}5\ Cu} = 0.171$

To find the correct time of exposure needed for giving 1 S. E. D. through, respectively, 5 mm Al and 0.5 mm Cu, we then substitute the above values in the formula, as follows:

$$T_5 = 1 \times \frac{39.8}{16.6} \times \frac{0.98}{0.37} = 2.5 \times 2.4 = 6$$

$$T_{0{\cdot}5\ cm.} = 1 \times \frac{39.8}{9.5} \times \frac{0.98}{0.171} = 4.2 \times 5.75 = 24$$

and those are precisely the exposure-times that we had found empirically.

In all our considerations concerning the precision of the various measuring instruments we have taken it for granted that the biological effect on the skin is a constant phenomenon, — just as was done by WINTZ, when he established the term S. E. D. (Skin Erythema Dose) and all those other, more problematical terms: »cancer dose», etc. Thanks to the high development reached by the roentgen-physical science, many roentgenologists have become accustomed to reason more as physical scientists than as physicians; still, to most physicians the idea of the skin effect as a *constant* phenomenon would probably a priori seem rather doubtful, considering how used they are to see how erratically both physiological and biological processes are in the habit of developing.

That there should be room for idiosyncrasies as regards the reaction to the roentgen rays is still denied, I believe; but that, for instance, the skin erythema dose is a highly variable quantity is probably becoming more and more generally accepted as true. To pronounce oneself authoritatively on that subject would, of course, require an experience extending over a great many years. Still, as our practice in the matter of dosage has been rather constant during the last twelvemonth, I believe I may be justified in calling attention to the result of our observations; which is that the skin erythema dose is, according to our impression, decidedly variable. In our conception as to what constitutes a skin erythema dose we have accepted the definition of it, given by WINTZ, as "the dose which will cause a slight erythema to appear within two weeks, followed by a medium strong pigmentation". Pigmentation without erythema

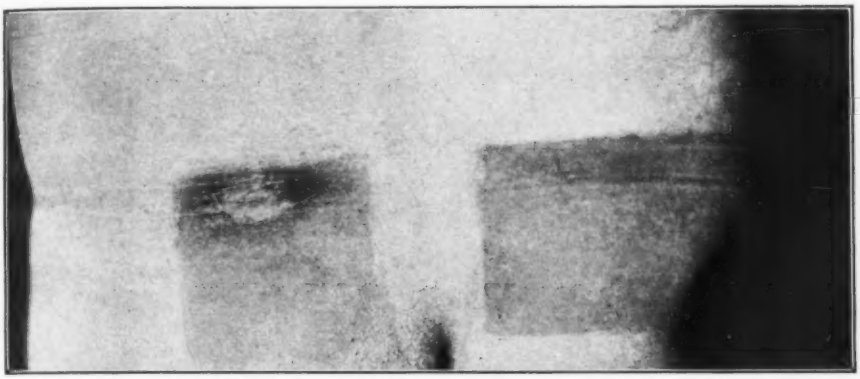

Fig. 9. Stronger reaction after X-ray treatment of skin area corresponding to the position of a truss (thin atrophic skin). Skin also much changed over an area corresponding to the truss-pad (dose 1 S. E. D.).

we have found resulting from much smaller doses; in some cases from as little as one fifth or one fourth of 1 S. E. D.

We have observed cases in which a patient, whom we believed we had given a dose of 1 S. E. D., did not show any reaction at all; so that, even up to two months after, it was hardly possible to distinguish the field; while in others the same irradiation produced an intense, almost blueish-red coloration of the skin, which later, however, gave place to a stronger pigmentation. In no instance, though, have we seen what might be called a reaction of the second degree, and much less any case of actual roentgen-burn. We have also in many cases noticed that there is a great difference between the reactions produced in the various fields. Thus, in cases of metrorrhagia, the reaction would generally be stronger on the anterior side than on the posterior. Also in the places where the skin lies immediately over some bone, the reaction would often be stronger. Likewise, we have had examples of the well-known fact that irritations of a different nature — such as painting with tincture of iodine, pressure of hernia-bandages, irritation in folds of the skin, etc. — are liable to increase the effect of the irradiation (fig. 9).

How great this difference in the effect on the skin may be, was illustrated to us one day, when a patient came in for after-examination two weeks after having been treated for a cancer of the rectum. The dose given had been 1 S. E. D., in four different fields, each field divided over two days. On examination, all fields showed a very strong blueish-red erythema; there could be no doubt but that the reaction had been decidedly too strong. We had hardly had

Fig. 10. Very strong reaction (too strong) after radiation with 1 S. E. D.

Fig. 11. No reaction of the skin after radiation with 1 S. E. D. (compare Fig. 10: the two patients are treated for the same complaint immediately after one another on the same day and with the same dose).

time to get over the disagreeable impression of this result, before another patient came in, who happened to have been treated — also for a cancer of the rectum — on the very same dates, and with the very same strength and manner of dosage, as the first one. But in this second case there was not the slightest sign of any reaction as regards the anterior fields, while the field over the os sacrum — above a bony structure, consequently — showed a mottled pigmentation, but no redness (fig. 10, fig. 11).

Fig. 12. Patient exposed to radiations of 1/2, 3/4, 1 and 1 1/4 S. E. D. to control our empirical S. E. D. That field having received the full dose, showed slight redness after 14 days, later moderate pigmentation; the other fields correspondingly weaker and stronger reactions.

In the way of control — as a means of finding out whether our skin erythema dose was reasonably correct or not — we divided the abdominal fields, in the case of a patient under treatment for metrorrhagia, in four areas, and subjected these, on the same day, to a dosage differing for each of them, as follows: to the 1st area, a dose which we considered to be equal to $\frac{1}{2}$ S. E. D.; 2nd area, $\frac{3}{4}$ S. E. D.; 3rd area, a "full" dose; 4th area, $1\frac{1}{4}$ S. E. D. The result — of which the accompanying illustrations unfortunately give a rather indistinct idea — was that the area which had received 1 full S. E. D. dose showed, after two weeks, a slight redness, increasing somewhat in intensity downwards toward the symphysis (bone); while the area that had received $1\frac{1}{4}$ S. E. D. was intensely red. Later on, the four areas showed four different degrees of pigmentation: the two first ones light, the third one medium, and the fourth one strong. From this we concluded that we had hit fairly correctly the skin erythema dose, which, as a matter of fact, we had found from the very beginning by means of the Sabouraud pastille (fig. 12).

In the beginning of this article I mentioned how the change from an accustomed apparatus to a newer and more powerful one often results in the operator's obtaining reactions decidedly in excess of what he has aimed at or expected, and how the same thing happened to us when we increased the tension from 150 kilovolts to 180 kilovolts. The case of cancer just described, with its excessive reactions, was precisely one of the first cases in which we employed a tension of 180 kilovolts. Subsequently we became more careful about giving the full dose. We had another case, in which conditions were somewhat different. It was a case of cancer mammæ, in which the treatment had hitherto been given with a focus-distance of from 40 to 50 cm. On account of some skin metastases we now gave a full dose — irradiating only a small field, at that — with a distance of 23 cm.; that is: we gave one third of the time for 40 cm. distance, and not the somewhat shorter time which sufficed for blackening the pastille at 23 cm.; and also in this case the resulting reaction proved to be too strong.

It is also a fact established long ago that the effect of an irradiation dose is more powerful when administered in one exposure than if the same dose is given by fractions, extending over a greater length of time. Thus, WINTZ, for instance, counts with a supplementary dose if the treatment extends over several days.

The highly interesting problem of the time-factor in roentgen treatment has been made the subject of a most instructive paper by HOLTHUSEN, published in the »Strahlentherapie», vol. XXI, no. II, p. 275.

It would exceed the limits of the present paper if I were to enter here on a discussion concerning the therapeutic value of concentrated treatment under high intensity. It is a gigantic problem in itself; all the more so since we know that the experiences of the »Régaud school» with long-time fractioned treatment, and the resulting better effect on the cells at their stage of mitosis, point in the opposite direction.

The relation between dose and effect rests primarily on the BUNTSEN-ROSKOE law which says, concerning photo-chemical processes, that »the effect is equal to the intensity multiplied by the time of irradiation» — in other words, a constant $K = I \times t$. This is also what we have been taking to be the case, right along, in making our observations and conclusions. Now SCHWARZSCHILD, however, calls attention to the fact that this law does not invariably hold good, and that it does not apply, for instance, to the darkening of the photographic plate; and he has therefore given it another form, which

he expresses by the formula: $I^q \times t = K$; q representing a constant which depends on the character of the photographic plate.

HOLTHUSEN has collected all the experiences in regard to roentgen-radiation that might tend to show that SCHWARZSCHILD's law applies to the latter. Among those experiences he cites the absolutely convincing test, made by ARNTZEN and KREBS, in which it is seen that the inhibitory effect on the growth of the peas was considerably stronger on those that had received the full dose in a single exposure of 24 minutes, than on those where the same dose had been given divided, in 3 exposures of 8 minutes each, with an interval of 12 hours between each.

HOLTHUSEN has himself made some experiments with eggs of the Ascarides. Some of these he subjected to a single exposure of 3 minutes; to others he gave the same dose in four instalments distributed over a period of 90 minutes. He noticed a distinct difference in the effect produced, and also that this difference seemed to be less the shorter he made the interval between the succeeding irradiations. He comes to the conclusion that BUNTSEN's law applies to the biological effect of the roentgen rays only to a limited extent: namely, as regards very short, definite exposures.

Even HOLTHUSEN's work does not by any means solve this difficult problem completely. Still, it may well be thought that some such conditions as indicated by him may have a determining influence in producing the stronger effect observed from the short-time exposures of great intensity. To obtain any standard measurement for this factor is, of course, impossible; but I should imagine that it would be prudent to dose more warily when going over to greater intensity and shorter exposure-times. I have also pointed out how the blackening of the Sabouraud pastille takes place more rapidly than commensurable with the square of the distance law. In so doing, it was the heat-effect that I more particularly had in mind; but I cannot help wondering — though it is merely a loose theory, of course — whether the SCHWARZSCHILD law may not, perhaps, have some bearing on the working of the Sabouraud pastille also.

I have so far, in this paper, dealt particularly with the surface dose, and with the problem of avoiding over-exposure with respect to the skin. That multiple-field irradiation is attended with considerable risk of over-dosage in the deeper parts is nowadays perfectly realized by most roentgenologists, thanks to HOLFELDER and others. HOLFELDER's »field-selector» gives a very clear and easily comprehensible picture of the distribution of the radiation throughout the body. If we nevertheless have refrained from using it, it is not only on account of its cost, which is considerable, but chiefly because we believe it to be the best principle that the matter of

dosage should rest, whenever possible, on the measurements of the operator himself. We have therefore by means of the water-phantom figured out the intensity, under every different condition of distance, filtration and size of field used by us, for every centimeter of depth, from the surface to 15 cm. penetration. Afterwards, we made the same measurements with the paraffin block; and finally we made working charts, on the transparent celluloid of old films, indicating the depth-percentage centimeter by centimeter. For the rest, our way of determining on the manner of irradiating is very much the same as the one used by HOLFELDER. If I dwell on these little details of our daily working-practice it is only in order to show that it is possible for the operator to establish a base of orientation for the whole of his deep-dosage work with very simple means and at a trifling cost. Nor does it, with the Siemens' dosimeter, take long time to make the necessary measurements for the purpose (fig. 13).

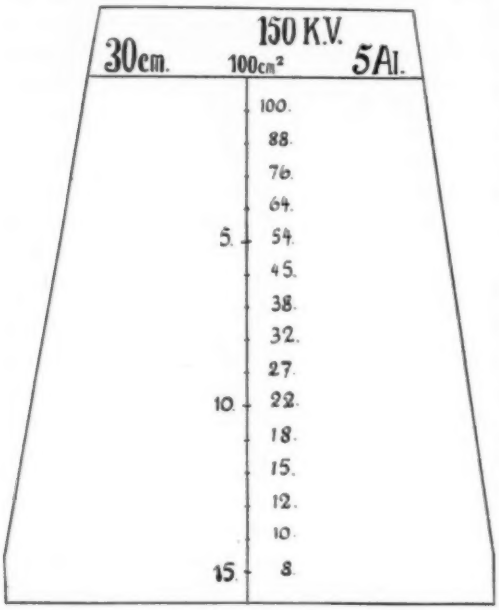

Fig. 13. Simple home-made scale-board, made of celluloid on which the results of one's own measurements are recorded. The percentage surface intensity for each cm. in depth is seen recorded. The scale board is applicable for 30 cm. focal distance, 150 KW, 5 Al filter and a field of exposure of 100 cm. (Scale-boards for all distances used, filters and sizes of fields, are made in the same way).

I shall mention a few figures from these measurements of ours. It is true that the conditions which they illustrate are already known; but I nevertheless believe they may prove rather instructive, at least to those who may not be in a position to undertake similar measurements themselves. As regards the question of distance, it is not so very much that is gained by increasing it. An increase of the distance from 23 cm. to 40 cm. only makes a difference of about 4 per cent. The influence of the filtration is greater. Between unfiltered radiation and radiation through copper the difference in effect, at a depth of 10 cm., is almost 100 per cent. It should be noted, however, that under

Table 3.

Penetration-percentage on water- (and paraffin)-phantoms.

Field	cm.	½ Cu	8 Al	5 Al	4 Al	3 Al	2 Al	1 Al	O F.
10 × 10	23	25		20					
	30	27	26	22					
	40	29		24					
10 × 15	40	31		27					
	50	34 (41)	(39)	28 (35)	27 (34)	26 (33)	24 (32)	22 (30)	18 (25)

(The figures in a parenthesis show the penetration-percentage on paraffin-phantom.)

Table 4.

Penetration-percentage dependent on the size of the field in paraffin-phantom						
Distance of 30 cm.						
Field	6.2 cm.²	9.4 cm.²	19.6 cm.²	39.2 cm.²	100 cm.²	196 cm.²
%	21	24	25	28	35	40

our conditions of experimentation even unfiltered radiation has a not inconsiderable penetration. Measured on the water-phantom the penetration-percentage for unfiltered radiation, at 50 cm. distance, is about 17 to 18 per cent.; measured on the paraffin block it is even as much as 25 per cent. I call attention, here again, to the fact that there is a considerable difference in the penetration percentage as measured, respectively, on water and paraffin. On the average we have found this difference to amount to about 7 per cent. I believe we shall not be very much wrong if we calculate the penetration-percentage for the human body to lie about midway between the figures for the two other substances here named.

The size of the field, as already mentioned, plays a very great rôle. We have measured the influence of this factor successively with all the variously sized radiation tubes ordinarily employed, and found, for instance, that with a small lead-glass diaphragm 2.8 cm. in diameter, and a distance of 30 cm., the penetration percentage was 21 or 22 per cent.; while with a field of 14 × 14 cm. the percentage was 40 per cent.

Finally, we have made a series of measurements of the same kind as those that were made by JAEGER some years ago, to determine the various degree in which the intensity is weakened in air and water, respectively. We did this more particularly in order to get a control for the capacity of our instruments to measure correctly.

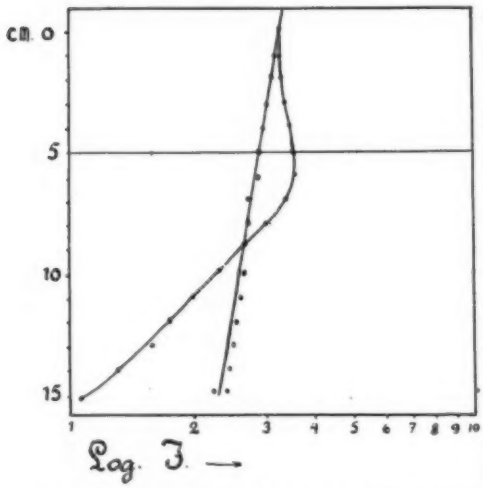

Fig. 14. Weakening of radiations in air alone and air and water (surface of water at 5 cm).

Inasmuch as the air does not scatter the radiation to any very perceptible extent, the weakening should follow the square of the distance law very closely. As it will be seen from the following curve, where the intensity values are stated in logarithmic units, the line is indeed the perfectly straight one which should be the result if the measurements are in conformity with that law (fig. 14).

These measurements in air and water are interesting, because they illustrate the fact that the spreading of the radiation, in water, at the same time as it is a weakening means a gain also. Our curves, which show the results of measurements both at 140, 150, 160 and 175 kilovolts' pressure — and which are absolutely identical with the ones published by JAEGER — show the interesting and highly instructive fact that already in the air above the water, the radiation is able to counteract the weakening, owing to the distance, and that we get the same dose both at the surface and at a depth of 1 cm.; which is, of course, something very important to know when it is a question of treating affections situated in the skin or immediately underneath.

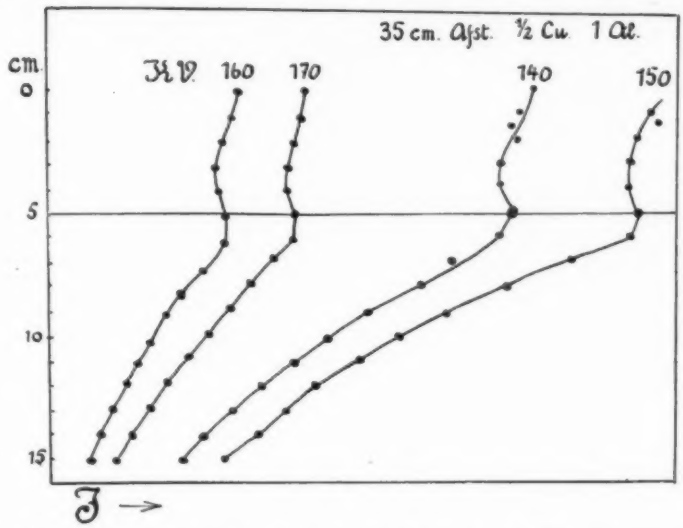

Fig. 15. Weakening of radiations in air and water (in a depth of 1 cm. the intensity of radiations is the same as the surface intensity. — Surface of water at 5 cm.)

SUMMARY

The present paper represents an attempt to surmount some of the difficulties, and answer some of the questions, with which the roentgen operator is faced whenever the substitution of a new and more powerful therapeutical apparatus, in the place of the old and feebler one with which he has hitherto been accustomed to work, obliges him to re-establish his entire scale of exposure-times to fit the altered conditions.

The author's measurements were made by means of the Siemens' Dosimeter and the Sabouraud Pastille, and by means of the Dosimeter in connection with a tube of platino-cyanide of barium, the latter being fitted over the ionisation chamber.

An investigation as to the various respects in which the more powerful apparatus presents an advantage results in the conclusion that, as regards the skin erythema dose, it means an enormous shortening of the exposure-time; while, on the other hand, for voltages higher than 100 or 120 k. v. its use does not result in any appreciable further increase of the penetration percentage.

The limitations of the various measuring devices are discussed, and particularly that principal shortcoming, attaching to them all, that there is no means by which the indications for the different qualities of radiation can be compared. Especially is the Sabouraud Pastille and its alleged deficiencies gone into very exhaustively.

A chief objection to the pastille being based on the fact that at certain wave-lengths its own characteristic radiation causes it to colour too quickly the author was induced — particularly as the result of earlier investigations by ARNTZEN and KREBS — to make some further studies concerning the absorption in the pastille itself. To this end he has measured, by means of the dosimeter, how much the intensity of the radiation became diminished when the ionisation chamber was entirely covered by a tube of Sabouraud-pastille paper. He has measured this absorption at increasing tensions of from 60 to 180 kilovolts, and also under various conditions of increased filtration at many different tensions; and he has found that there is a perfectly gradual decrease in the absorption following filtration with from 1 mm. Al to 8 mm. Al and $^1/_2$ mm. Cu + 1 mm. Al — the last-named composite corresponding to 11 or 12 mm. Al. He has, thus, been unable to find any break in the absorption which might be explained as due to the characteristic radiation.

It is taken for granted that the erythema dose is the resulting product of time × intensity of radiation × the percentage of radiation which is absorbed by the skin; and if it were possible to determine the value of each of those factors separately it would be possible to figure out the correct exposure-time for a different quality of radiation. It is supposed that the absorption in the pastille-paper tube, measured as just indicated, is parallel with the absorption in the skin, and that the ionisation current is an expression of the surface-intensity on the skin. On this basis it is tried, by means of a simple equation, to find the correct exposure-time for 1 S. E. D. under various conditions of intensity of radiation. Practical tests, in which the exposure-times for 1 S. E. D. had previously been determined empirically, seem to prove the correctness of those theoretical conclusions.

Practical experiences concerning the erythema dose prove the latter to be somewhat different for different individuals; and it has been observed, also, that the reaction of the skin becomes unexpectedly strong under short-time exposure with higher intensity. These phenomena the author likewise attempts to account for.

Numerous measurements have been made by the author, both on water- and paraffin-phantom, under all the different conditions of radiation employed at his clinic. Those measurements are recorded on home-made celluloid working-charts, by means of which it is possible to establish exact plans for the dosage in each case.

ZUSAMMENFASSUNG

Die vorliegende Arbeit ist ein Versuch, einige von den Schwierigkeiten zu überwinden, und einige von den Fragen zu beantworten, welchen der Röntgenologe gegenübersteht, so oft ihm die Ersetzung des alten und schwächeren therapeutischen Apparates, mit dem er bis dahin zu arbeiten gewohnt war, durch einen neuen und wirkungsvolleren zwingt, seine ganze Skala von Expositionszeiten den geänderten Bedingungen gemäss neu aufzustellen.

Die Messungen des Autors wurden mit Siemens Dosimeter und Sabouraud-Pastille ausgeführt, sowie mit dem Dosimeter in Verbindung mit einer Röhre von Barium-Platino-Zyanid, welch letztere über die Ionisationskammer geschoben wurde.

Eine Untersuchung hinsichtlich der verschiedenen Beziehungen, in welchen der kräftigere Apparat einen Vorteil bietet, führt zu dem Schlusse, dass er betreffs der Haut-Erythemdosis eine enorme Verkürzung der Expositionszeit bedeutet; während er für höhere Voltzahlen als 100 oder 120 k. v. zu keiner merklichen weiteren Steigerung der Penetrationsprozentzahl führt.

Verfasser erörtert die Begrenzungen der verschiedenen Messungsvorrichtungen und besonders die allen anhaftende hauptsächliche Schwäche, dass es kein Mittel gibt, die Ausschläge für die verschiedenen Strahlungsgattungen zu vergleichen. Besonders die Sabouraudsche Pastille und die ihr zugeschriebenen Mängel werden sehr eingehend besprochen.

Da sich ein Haupteinwand gegen die Pastille auf die Tatsache gründet, dass für gewisse Wellenlängen eine zu rasche Färbung der Pastille durch ihre eigene charakteristische Strahlung verursacht wird, fand sich Verfasser veranlasst — speziell in Anbetracht der früheren Untersuchungen von Arntzen und Krebs einige weitere Studien über die Absorption in der Pastille selbst zu machen. Zu diesem Zweck hat er mittels des Dosimeters gemessen, um wieviel die Strahlungsintensität verringert wurde, wenn die Ionisationskammer vollständig mit einer Röhre von Sabouraud-Pastillen-Papier bedeckt wird. Er hat die Absorption bei steigenden Spannungen von 60 bis 180 Kilovolt gemessen und auch unter verschiedenen Bedingungen von gesteigerter Filtration bei vielen verschiedenen Spannungen. Dabei fand er eine völlig regelmässige Abnahme der Absorption bei Filtration von 1 mm Al bis zu 8 mm Al und $^1/_2$ mm Cu + 1 mm Al, welch letztere Kombination 11 oder 12 mm Al entspricht. Er war also nicht imstande, eine Unterbrechung in der Absorption zu finden, die auf die charakteristische Strahlung zurückgeführt werden könnte.

Es wird als ausgemacht angenommen, dass die Erythemdosis das resultierende Produkt von Zeit × Strahlungsintensität × Prozentanteil der durch die Haut absorbierten Strahlung ist; und dass es möglich wäre, die korrekte Expositionszeit für eine differierende Strahlungsgattung zu berechnen, wenn man den Wert jedes dieser Faktoren separat zu bestimmen vermöchte. Unter der Supposition, dass die auf die eben angegebene Art gemessene Absorption der Pastillen-Papierröhre der Absorption in der Haut parallel ist, und dass der Ionisationsstrom ein Ausdruck der Oberflächenintensität an der Haut ist, versucht Verfasser, mittels einer einfachen Gleichung die korrekte Expositionszeit für 1 H. E. D. unter verschiedenen Bedingungen betreffs der Strahlungsintensität zu finden. Praktische Proben, bei welchen die Expositionszeit für 1 H. E. D. vorher empirisch bestimmt wurde, scheint die Richtigkeit dieser theoretischen Schlüsse zu beweisen.

Praktische Erfahrungen betreffs der Erythemdosis zeigen, dass die letztere für verschiedene Individuen etwas variiert; auch wurde beobachtet, dass die Hautreaktion bei kurzdauernder Exposition mit höheren Intensitäten unerwartet stark wird. Diese Phänomene versucht der Autor gleichfalls zu deuten.

Verfasser hat sowohl an Wasser- als an Paraffin-Phantomen unter all den verschiedenen Strahlungsbedingungen, die an seiner Klinik verwendet werden, zahlreiche Messungen vorgenommen. Diese Messungen sind auf selbstverfertigten Zelluloid-Arbeitstabellen verzeichnet, welche es ermöglichen, in jedem Falle einen exakten Dosierungsplan aufzustellen.

RÉSUMÉ

Dans la présente communication, l'auteur essaie de répondre à quelques-uns des problèmes, et de surmonter quelques-unes des difficultés, qu'éprouve le radiologiste opérateur chaque fois que la substitution d'un nouvel appareil thérapeutique plus puissant, à la place de l'ancien appareil plus faible dont il avait acquis la pratique, l'oblige de rétablir entièrement l'échelle de ses temps d'irradiation, de façon à les mettre en rapport de conformité à ces conditions nouvelles.

Comme procédés de dosimétrie, l'auteur a employé le dosimètre de Siemens et la pastille de Sabouraud, et le dosimètre combiné avec une espèce de couvercle fait de papier au platino-cyanure de baryum, pour couvrir la chambre d'ionisation.

Ayant cherché de connaître les différents avantages offerts par l'appareil plus puissant, l'auteur conclut qu'en ce qui concerne la dose érythème il permet d'abréger énormément le temps d'irradiation, tandis que, pour les voltages au-dessus de 100 ou 120 Kv, il n'accroît pas sensiblement le pourcentage de la pénétration.

Il constate l'insuffisance des différents procédés de mensuration, et notamment ce défaut sérieux, qu'ils ont en commun, de n'offrir aucun moyen par lequel il soit possible de comparer les indications pour les différentes qualités de rayonnement. Surtout, il discute en grand détail la pastille de Sabouraud et les imperfections qu'on lui reproche.

Comme une critique essentielle contre la pastille repose sur le fait qu'à certaines longueurs d'ondes son rayonnement caractéristique le fait changer de couleur trop rapidement, l'auteur fut porté à pousser plus loin l'étude — déjà abordée par Arntzen et Krebs — de l'absorption dans la pastille même. Dans ce but il a mesuré, au moyen du dosimètre, combien diminua l'intensité du rayonnement, quand la chambre d'ionisation fut entièrement couverte d'une enveloppe de papier-pastille Sabouraud. Il a mesuré la quantité de cette absorption sous des voltages variant de 60 à 180 Kv, ainsi qu'avec des épaisseurs progressives de filtre sous une grande variété de voltages; et il a constaté une diminution parfaitement graduée de l'absorption à mesure que la filtration est augmentée de 1 mm d'aluminium jusqu'à 8 mm d'aluminium, ou à 0.5 mm de cuivre + 1 mm d'aluminium, — cette dernière composition correspondant à une valeur de 11 ou 12 mm d'aluminium. Il n'a donc pu constater, par conséquent, la moindre discontinuité d'absorption dont la présence s'expliquerait comme étant le résultat du rayonnement caractéristique.

Admettant que la dose érythème est le produit du temps de pose multiplié par l'intensité de rayonnement, multipliés par le pourcentage de rayonnement absorbé par la peau, le temps d'érythème propre à une différente qualité de rayonnement se laisserait facilement calculer, si seulement il fut possible de déterminer, séparément, la valeur de chacun de ces trois facteurs. L'auteur, alors, part de la supposition que l'absorption dans l'enveloppe de papier-pastille — mesurée de la manière indiquée — est parallèle à l'absorption dans la peau, et que le courant d'ionisation exprime l'intensité du rayonnement à la surface de la peau; et, sur cette base, il a essayé de trouver, au moyen d'une simple équation, le temps de pose nécessaire pour donner 1 dose érythème (1 H. E. D.) sous différentes conditions d'intensité du rayonnement. Des expériences pratiques, dans lesquelles le temps d'érythème avait été dé-

terminé d'avance, empiriquement, paraissent confirmer pleinement ces conclusions théoriques.

Les expériences pratiques ont prouvé que la dose érythème est quelque peu variable d'un individu à l'autre; encore a-t-on observé que la réaction de la peau devient violente outre mesure quand l'irradiation est pratiquée en temps abrégé avec augmentation de l'intensité. Ces phénomènes, également, l'auteur essaie de les expliquer.

L'auteur a fait de nombreuses mesures, sur fantômes d'eau et de paraffine, sous toutes les différentes conditions d'irradiation qu'il a rencontrées dans sa clinique. Comme résultat de ce travail il a pu fabriquer, pour l'usage de cette dernière, toute une série de cartes-échelle permettant de calculer d'avance la dose exacte à donner dans chaque cas.

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PROCEEDINGS OF THE DUTCH SOCIETY FOR ELECTROLOGY AND RÖNTGENOLOGY

50th General Meeting on Sunday 1 March 1925

President: Dr. L. F. Driessen.
Secretary: Dr. N. Voorhoeve.

I. A. Bouwers: *About Measuring of the Intensity of X-Rays.* (Published in the Acta Radiologica Vol. IV, 368.)

Discussion. van Ebbenhorst Tengbergen asks whether the exponent p in the formula of Schwarzschild is high or low and what is the reason, why in ordinary photography we do not observe any trace of the existence of this exponent. Is it not possible to put a blackening scale on each plate, so that we could expose on different plates and compare these together?

Bouwers answers, that p is always about $^9/_{10}$ for ordinary light and the reason, that we do not observe anything of this exponent in ordinary photography is, that it is very difficult to find out by approximative methods if any difference exists between p and the unity, when this difference is very small; therefore they formerly thought, that for sunlight p equalled 1 and it was only on close examination that this was found not to be the case. The use of two plates for measuring the blackening proves in practice unsatisfactory; even when a blackening scale is applied differences are found to amount to about 20 %. The objections to measuring on one plate, are not so great as might be supposed.

Timmer asks, if the blackening of the photographical plate in the case of very hard rays is also proportional to the square of the tension of the tube? Gortan showed beautiful negatives, made with hard rays, at the German Röntgencongress 1924. In the discussion some people thought, we ought to use harder rays than commonly and Weber thought, that the times of exposure do not diminish with the square, but with the third power of the tension. Holzknecht and others could not explain the results of Gortan. Could speaker perhaps give any explanation?

Bouwers answers, that Franke and others have given the following explication as to the radiographs with hard rays: it is a fact, that also with hard rays the blackening is proportional to the square of the tension, but as the time of exposure is proportionally decreased, the blackening is less and with a little blackening, contrasts are better visible.

Heilbron asks, if speaker did not find trouble from the fact, that the alloy of bromide of silver in the layer of a photographic plate is not homo-

geneous. He thinks that this fact even sometimes causes trouble in radiographical work.

BOUWERS answers, that this may occur a single time, but is not often seen. The alloy of bromide of silver of the different plates appeared to be very constant and showed differences of only a few percents. Chemical examinations of arbitrarily chosen films gave similar results; the photographic plate is more homogeneous, than one would think.

VOORHOEVE points to the fact, that there is difference of action between röntgen rays on a plate and on living tissue. The action of röntgen rays of a definite wavelength on the photographic plate may be exclusively dependent on the product of intensity and time (i. e. the exponent p in the formula of SCHWARZSCHILD is equal to the unit and the law of BUNSEN is applicable), we all know, that the law of BUNSEN cannot be applied to the biological action of röntgen rays. The difficulty is, that we have to consider here the exponent p without knowing anything of its real value. Now speaker's supposition, that for the photographical effect of röntgen rays the exponent p is equal to the unit, whereas for ordinary light it is not, because röntgen light is an intermittent light and ordinary light is continuous, raises the question, if besides the experiments with intermittent ordinary light, also experiments have been made with high tension continuous current on the röntgen tube. In the latter case, the röntgen rays being also continuous, p ought to be smaller than the unit. He asks speaker, if, when really such experiments have confirmed the abovementioned supposition, it may not be wrong to use the photographical plate for measuring the intensity of röntgen rays produced by high tension continuous current?

BOUWERS answers, that experiments as mentioned by VOORHOEVE, have been made and that they have confirmed the suppo-ition, that p is smaller than the unit in case of continuous current, but there is no objection to use the photographic plate for measuring intensities, because p does not become smaller than 0.94 and it is also easy to apply corrections for alterations of the value of p.

II. J. VAN EBBENHORST TENGBERGEN: *Demonstration of a Potter-Bucky-Diaphragma with Fixed Tubestand.*

III. D. J. STEENHUIS: *About a Special Case of Ostitis condensans disseminata.*

Speaker shows the radiographs of the bones of a girl of thirteen years of age, who was suffering from her abdomen. In the epiphyses and in some parts of the diaphyses small spots of different sizes, just as described for the first time by ALBERS-SCHÖNBERG, were to be seen. Laparotomy showed the existence of little pieces of tissue about the size of a pea spread over the whole peritoneum. Microscopical examination of one of these pieces seemed to point to tuberculosis. Speaker thinks it is possible, that the spots in the bones may also have been due to tuberculosis.

Discussion. VOORHOEVE points to the somewhat strange localisation of the spots, the metaphysis containing only a few of them. It is still doubtful, as speaker observed himself, whether this alteration is identical with the one described by ALBERS-SCHÖNBERG and others. For that purpose, it is of interest to know, whether the round spots lie all over the spongiosa or against the inner or outer side of the corticalis. Has speaker examined his patients stereoscopically or has he used other methods of localisation? The fact, that

in this case, the alterations in the skeleton occur together with a doubtful tuberculosis of the inner organs, can hardly be used to prove a tuberculous origin of the alterations in the skeleton; tuberculosis is a very common affection and therefore might easily be found in patients with these alterations in the skeleton. Only one of the 6 cases mentioned in literature, namely that of MOREAU had tuberculosis (vertebrae). He does not agree with speaker, that the presence of a dark spot in the diaphysis, precludes the fact, that the dark spots may be caused by calcification of cartilage in abnormal places. We find just the same difficulty with the cartilaginous exostoses. Now and then we may see an exostosis in the middle of a diaphysis. And still the opinion is generally held, that the cartilaginous exostoses of the diaphysis arise from a primary disturbance, which takes place during the growth of the bone and which causes cartilage to appear, where bone ought to exist. This even remains true, when one accepts the views held by those investigators, who like MÜLLER do not seek the primary disturbance in the intermedian cartilage, but in an abnormal production of cartilage by the periost and the perichondrium.

STEENHUIS replies that he has not yet examined the situation of the dark spots with regard to the corticalis. The presence of a dark spot in a diaphysis remains for him an objection to accepting a disturbance in growth.

HEILBRON saw the same kind of spots in a foetus of 4 months.

KEYSER saw a similar case in a child a few days old. The spots were also seen in the spine.

DRIESSEN asks how far the microscopical examination of the small tumours taken out of the peritoneum was typical for tuberculosis. Macroscopically they do not look typical. Did the child suffer from rickets?

JANSEN asks whether the alterations were everywhere symmetrical.

STEENHUIS confirms this. The child had not got rickets; the microscopical examination was not typical for tuberculosis, but tuberculosis was most probable.

IV. N. VOORHOEVE: *On Granuloma malignum.*
(Published in the Acta Radiologica, Vol. IV, 567.)

Discussion. GAARENSTROOM asks if speaker's experience also agrees with his, that all these patients at last die of their sickness. It is found in literature, that in 40 % of the cases granuloma malignum is accompanied by tuberculosis. What is speaker's experience?

STEENHUIS radiates daily one or two hours and asks if speaker also applies large doses in a short time. Did he see many metastases in the skull and did it also strike him, that in cases of relapses, the glands are sometimes harder and more solid than at the beginning.

KEYSER saw a protusio bulbi arise in 3 patients. One of them showed a destruction of the sella turcica. He treated a babe of 8 to 9 months, which had shown no relapse for years.

VOORHOEVE answers that, up to now, he has not seen a permanent cure; as he has said, the longest time, that one of his patients lived from the beginning of the treatment, was $6^1/_2$ years. In the cases of the 4 patients still alive, the treatment began $4^1/_4$, $3^1/_2$, $3^1/_4$, $^1/_2$ years ago. Three of them are now in good health. He purposely did not speak about the combination with tuberculosis, neither about the technic of radiation, the time being too short to speak on these and other important subjects. Tuberculosis was verified twice, once in the case mentioned, showing a number of bacilli in the sputum,

another time in the case, where the examination of an extirpated gland showed tuberculosis and the post mortem examination showed tuberculosis as well as granuloma malignum. As to radiation he likes to individualise his cases, applying as soon as possible the necessary dosis on the necessary place. He must warn against repeated appliance of parts of an erythemadosis. It seems to him, that it is neither necessary nor desirable to give such large doses in so short a time as STEENHUIS does. It is seldom necessary to apply an erythemadosis upon more than one area a day. And such a dosis may be administered in 10 to 15 minutes.

He saw metastases in the periost of the bones of the skull in a few cases. Once with a large protuberant tumor of the affected bone; all of them disappeared after radiation. In cases of recurrence the affected parts often show infiltration. Microscopical examination of these cases sometimes shows the tumor growing through the capsula of the gland, resembling a sarcoma. Nevertheless we may not conclude from the fact, that a group of glands is hard and solid, that the tumor is of an infiltrative growth, especially if radiation has taken place before, as generally is the case in recurring affections. We ought always to think of the possibility of an induration of the subcutis, caused by radiation, especially when radiations have been applied at too short intervals.

V. J. B. POLAK: *a. A Surprising Radiograph of the Chest.*

Speaker demonstrates a radiograph of the chest of a girl of 21 years, complaining of weakness and anorexia; subfebrile temperature. No tuberculosis in the family, no physical alterations in the lungs. After resting in bed for a short time, temperature becomes normal. No reaction on an injection of $^1/_{10}$ cc tuberculine Beraneck $^1/_{1000}$; Wassermann-test negative.

The röntgen examination showed a dissemination of miliary foci, spred over both lungs.

Such cases are rare. ASSMANN relates only 3 cases, one of which he saw from the start up to the time it healed. This patient died afterwards of urogenital tuberculosis and the post mortem examination showed in the lungs large quantities of incapsulated foci with central necrosis. HEINEKE (Beitr. zur Klinik der Tuberculose, Bd. 41) described a similar case with the same post mortem results. KÄSTLE (in SCHITTENHELM's book) also mentions such cases and RIBBERT examined such miliary foci anatomically.

With the prognosis we must be prudent. KÄSTLE says, that such patients sometimes get tuberculosis of the larynx. Speaker's patient is now quite well since 5 months.

Discussion. VOORHOEVE observes, that in 1917 DIETZ demonstrated a similar case. In connection with speaker's short summary of the literature on this subject, he should like to call to memory the communication of a fellow-countryman. The ample discussion, that followed, showed how afraid the members were to make the diagnosis of miliary tuberculosis. The remarks made at that time, when such images were not yet known in literature, are still to a great extent applicable to day.

J. B. POLAK: *b. A Few Remarkable Radiographs of the Stomach.*

1. The first radiographs are of a lady of 51 years. In Nov. 1915 she had a haematemesis, prior to which she had often suffered from pains in the stomach, both during the day and during the night, which sometimes dis-

appeared on her eating something. In her 18th and 29th year she also suffered from haematemesis. In June 1916 she was operated upon, when a small duodenal ulcer was found and a gastro-enterostomia was made next the pylorus. A short time before this radiograph was made she had complained of violent pains in the abdomen. In the right and left side of the abdomen tumors could be felt. She was emaciated and felt weak.

The röntgen examination showed a well acting gastro-enterostomia and a closed pylorus. Distal from the anastomosis there was a rather round filling-defect of the stomach. This defect and the clinical symptoms resembled most a case of cancer; however she had a total acidity of 38, HCl. 22. The borders of the defect were too smooth. After röntgen examination she was operated on, the stomach was found to be surrounded by adhesions, extending over the gallbladder, intestines and the urinebladder. At the place of the fillingdefect, just before the pylorus, a large radiated cicatrix was found at the ventral side of the stomach towards the small curvature; at the large curvature perigastritic adhesions were seen like a fringe. Cancer was not found.

We must assume, that after the first operation, an ulcer developed at the small curvature, causing an hourglass-stomach with extensive perigastritis. The fact that after a gastro-enterostomia for duodenal ulcer, a gastric ulcer may arise, causing an hourglass-stomach, throws a peculiar light on the surgical treatment of ulcers.

2. A man of 46 complained of pain in the upper part of the abdomen and in the chest, especially when walking. The pain awoke him during the night, but could be suppressed by eating. Before eating anything the stomach contained juice of high acidity. At first there was blood in the stool, but he had gums, which easily bled.

The radiograph showed a deformed antrum and a niche at the small curvature with a contraction of the large curvature at the opposite side. In the proximal part of the so formed hourglass-stomach intense peristalsis was seen. After 3 hours there was only a little quantity of food in the stomach, after 6 hours the stomach was empty.

An hourglass-stomach caused by an ulcer so near the pylorus, with such large difference between the sizes of the two parts of the stomach, is very rare. SCHWARZ gives an image of a similar case on page 879 of part II of the book of SCHITTENHELM in which originally the distal part was taken for an overfilled bulbus. The localisation is more common for cancer and cancers with niches are described, but the acidity argues against cancer; an ulcer is most probable.

3. Duodenum cases.

a. There is a great difference in opinion as to the frequency of niches in cases of duodenal ulcer. ÅKERLUND's book leaves the impression, that niches may be found in most cases of duodenal ulcer. I think they are rather rare in Holland. I myself could only find 6 distinct niches and 2 dubious bulbus niches in 200 examinations of the stomach. One was seen in a woman of 31 with typical hungerpains and with distinct hypersecretion and hyperacidity (free HCl. 54, total acidity 70). She showed hyperperistalsis of the distal part of the stomach and a bulbus, difficult to be filled. At the medial recessus was a sharp projecting part; when the patient was lying face downwards it changed into a round opaque spot. Perhaps it is not right to call such a deflection a niche. SCHWARZ uses the word niche only in cases of real penetration, the protrusion not being covered with normal mucosa.

In our case there is very likely a little pocket formed by cicatricial retraction, a »Tasche» according to SCHWARZ.

b. A man of 46 complained since about a year of vomiting of large quantities. Only the last three weeks, eating only bread and eggs, he did not vomit. Ten years ago he had also vomited for some time. Chemical examination: 40 cc. of juice out of the fasting stomach with little acidity containing remains of plants. After test-breakfast about 100 cc. containing free HCl. 63, total acidity 78. The first time blood was found in the stool, the second time none. Radioscopically the stomach filled itself normally, no dilatation was seen. After effleuration the duodenum could be filled, showing the pars descendens dilated. The bulbus was large with smooth borders. The lower part of the dilated duodenum did not show Kerckring folds. After 6 hours the same image, but now the opaque meal had sunk to the lower part; in the pars descendens there was a fluid. The patient had a stenosis of the duodenum at the junction of the duodenum and the jejunum. It struck us that, though the alteration already had existed for a year accompanied by retention-vomiting, the stomach was not dilated. The Kerckring folds had disappeared by pressure. Now NANDOR RATKOCZIN in the Americ. Journal of Röntgenology Sept. 1924 points to this symptom as constituting a difference between the intense duodenumfilling in cases of insufficient pylorus and stenosis of the lower part of the duodenum. He distinguishes between a continuous and an intermittent stenosis. The latter may be caused by pressure of the mesenterium or of the art. mesent. sup. In DUVAL ROUX and BÉCLÈRE (Etudes médico-radio-chirurgicales sur le duodénum) we find the same distinction. The authors even mention a stenosis which disappears when the body is in certain positions. ASSMANN points out the fact, that often in the dilated duodenum the meal is much diluted by gall and pancreas-juice, so that the part that only contains fluid, is almost invisible. Operation in this case did not take place, so that speaker cannot give any affirmation as to the anatomical alterations.

c. A man of 37 complains of pains about $1^1/_2$ hours after eating. By eating something or by drinking hot milk he finds relief, but the pain always returns till he takes a warm meal. When remaining too long in the morning without eating he has pains. Test-breakfast gives 100 cc. juice with 92 total acidity and HCl 75. No blood in the stool. Radioscopically and on the radiograph it strikes us, that the stomach lies far to the left. The stomach is narrow, orthotonic, the pylorus lies in the middleline. The bulbus completely filled is extraordinarily large and shows a great contraction at the side of the large curvature. The whole of the duodenum is distinctly visible. After $3^1/_2$ hours there are slight remains in the antrum and in the bulbus. The latter shows distinctly a contraction of the large curvature. After 6 hours the stomach is empty and remains are seen in the bulbus. Is this a case of a stomach lying far to the left owing to ptosis, with a ptotic bulbus and an ulcus of the small curvature, causing an hourglass-bulbus? Or is this a case of stenosis, caused by an ulcus (or cholecystitis) in the pars superior duodeni, with dilatation between bulbus and stenosis? SCHWARZ on page 880 of the second part of SCHITTENHELM's book gives a radiograph of such a case, very like our case. An ulcus is most probable on account of clinical symptoms and of the hypermobility of the stomach together with the fact, that remains were found in the bulbus after 6 hours. The hypermobility argues against the stenosis, so that the first assumption, an hourglass-bulbus in a case of ptosis, seems to me most probable.

VI. S. KEYSER: *Swallowed Pin with Imminent Mediastinitis.* (Will be published in the Acta Radiologica.)

VII. W. CHR. VAN DER LINDEN: *Explanation of Radiographs.*

a. A case of malacy of the os lunatum carpi.

b. A case of KÖHLER's disease of the os naviculare pedis.

c. A case of KÖHLER's disease of the second metatarsophalangeal joint of the left foot in a girl of 15.

The clinical, radiological and microscopical symptoms of all these affections are discussed and the current opinions on the pathogenesis are referred to. In all these affections we find necrosis of the bone, both right and left affected, the rest of the skeleton remaining free, several members of one family being often attacked.

The probability of all having the same cause impresses itself upon us. An individual predisposition, caused by disturbances of internal secretion, or a failure in development may be regarded as probable, whilst slight traumata play an important rôle in the origin of the complaints.

Discussion ad a: TIMMER demonstrates slides of a case of necrosis of the os lunatum in a carpenter of 20, who, one night after his daily work, felt violent pain in the right wrist. These radiographs were made a year later, the pain not having disappeared after treatment with bandages of starch. The alterations of the form of the bone are always the same in this kind of disease. DESTOT thinks that such cases are old compression fractures, he has never seen a fresh case. The experiments of AXHAUSEN and of KONJETZNY, who examined 8 cases of so-called malacy of the os lunatum has taught us, that they show a subchondral necrosis of the bone. What is the cause of this necrosis? AXHAUSEN thinks an obliteration of the vessels by non-infectuous bacterial emboli; KONJETZNY says, it is due to endarteritis obliterans. TIMMER thinks the trauma plays an important rôle. We can imagine that heavy manual work may cause a distortion of the carpus, a distorsio naviculo-navicularum, not being felt at the same moment. This distortion of the hand may be accompanied by rupture of the vessels, causing necrosis of the bone. When we consider, that the carpal bones lie almost intra-articular and that they are fed by vessels lying in the joining ligaments, then we can understand, that necrosis may arise in the above mentioned way. The necrotical bone may gradually change its form, when the patient begins to work.

VOORHOEVE observes that in 1910 PREISER already drew attention to the importance of injuries of the vessels in the joint-ligaments for the pathogenesis of these affections within the bone. He already at that time gave a similar explanation as now mentioned by TIMMER. As to the little light spot, that is sometimes found in the scaphoied bone and may be followed by a fracture, he especially will call to memory, that PREISER has argued, that it may be caused by a disturbance of alimentation of the bone following rupture of vessels. It struck VOORHOEVE that the first meta-carpal bone of the demonstrated hand, shows the image of an enchondroma. This might be of great importance for the aetiology of the lunatum affection. Has speaker thought of the possibility of some connection existing between those two affections in the same hand? Has it been possible to examine the tissue of the affected metacarpus microscopically or otherwise?

VAN DER LINDEN has not considered the question mentioned by VOORHOEVE, but he will study the case further.

d. Demonstration of an aneurysma of the abdominal aorta with destruction of several vertebrae.

In July 1924 radiographs were taken of a patient, which show in a ventro-dorsal direction a very large shadow, reaching from Th 9 to Th 12. On the right side the border of this shadow passes sharply over the corpus of Th 9, in contradiction with what we are used to see in cases of a paravertebrae-abscess. There is no important osteoporosis of the spine. In lateral direction we see destruction of the corpora of the vertebrae, beginning at Th 9; Th 11 and Th 12 are mostly destroyed, the more distal vertebrae show less destruction. Th 11 and Th 12 are pressed together, there is an angular kyphosis. No osteoporosis.

In February 1925 on a second examination, the shadow is found larger and the destruction of the vertebrae has increased.

Discussion. DE WILDE should not like to exclude tuberculosis of the vertebrae with gravitation-abscess.

HEILBRON excludes tuberculosis of the vertebrae; the most caracteristic symptom of tuberculosis of the spine, the affection of the intervertebral cartilage being absent. Another question is, if it is not possible, that we have to do with a sarcoma of multiple vertebrae. He should like to know, if there was pulsation.

KOPP also rejects tuberculosis; a gravitation-abscess originating in the lumbarspine has another shape and spreads downwards along the psoas.

VAN DER LINDEN neither thinks of the possibility of tuberculosis. Pulsations could not be felt, the patient was rather stout. Contemplating the whole case clinically, a sarcoma becomes most improbable.

VOORHOEVE observes that such cases are very rare in literature. As far as he knows only one case is described of an aneurysma of the abdominal aorta, proved by röntgen examination. He himself saw a case that is not published. The aneurysma was palpable and pulsated and showed on the plate calcifications of the wall and destruction of one vertebra. The post mortem examination confirmed the diagnosis. Speaker's case is probably a case of aneurysma, although the possibility of a sarcoma cannot be denied, and it may appear strange, that in the case of such a large aneurysma no pulsations could be felt.

KOPP thinks thrombosis may be the reason of the absence of pulsations. He thinks an aneurysma most probable.

VAN DER LINDEN. The retro-peritoneal situs of the aneurysma together with the stoutness of the patient and the possible existence of a thrombus are sufficient to account for lack of pulsations.

VIII. L. G. HEILBRON: *Differential Diagnosis between Tumor and Tuberculosis of the Spine.*

It has always been a hobby of speakers to demonstrate that in cases of sarcoma the intra-articular cartilage is never affected. On the contrary, tuberculosis tends to destroy it. Speaker demonstrates this by a number of radiographs. It has struck him that last year in France by SICARD this fact has been demonstrated with much emphasis, as something new in cases of destruction of the spine. Speaker therefore has studied the matter in röntgen literature and was surprised to find that so little stress was laid on this fact in German and English books. He thinks it might be of use to demonstrate

some cases, which could confirm this law of different resistence of cartilage to tumour and tuberculosis.

Discussion. TIMMER observes that not only tuberculosis destroys intervertebral cartilage. Destruction is also seen in KÜMMELL's disease. We may say, when the intervertebral discs remain unaffected and the bodies of the vertebrae are destroyed, there is a tumour, but we must add that, when the discs are flatter, we can only say there is no tumour.

DRIESSEN asks if the same is the case with osteochondromata.

HEILBRON answers that he thinks so, but he has no experience as to this.

51st Meeting on Sunday 17th May 1925.

President: Dr. L. F. DRIESSEN.

Secretary: Dr. N. VOORHOEVE.

I. J. VAN EBBENHORST TENGBERGEN: *The pressure in the canalis cerebro-spinalis in relation to encephalography.*

As experience with encephalography has frightened many experimentors, I have tried to find the reason of the alarming symptoms that sometimes occur. Lumbarpunction is to be preferred to ventricle-punction, as being much simpler. But just the lumbarpunction gives the disagreeable symptoms, caused by the change of pressure during operation. Pressure in the canalis cerebro-spinalis depends on:

1:o. The space at disposal.

2:o. The quantity of brain tissue.

3:o. The elasticity of the enclosing tissue.

4:o. The state of the vesselsystem, depending on bloodpressure and perhaps on an active dilatation or constriction of the vessels.

5:o. The quantity of liquor present at a definite moment; therefore on the long run on the relation existing between secretion and resorption.

6:o. The pressure from outside upon the foramina spinalia.

When we find by lumbarpuncture a pressure of 10 cm. in a patient, occupying a recumbent position this same pressure will exist in the hole canalis cerebro-spinalis. Leaning forward in a sitting position we will find a higher pressure of about 25 cm.; when the head is held up, pressure will increase by about 6 cm.

Fig. 1 represents a sitting up patient with a pressure of 30 cm. at the base of the spine. 30 cm. higher up, at about the basis cranii, pressure will be 0 and still higher up in the ventricles about — 10. This is the case, when all the parts of the canalis cerebro-spinalis communicate with each other and have nothing to do with the fact that we have here an elastic system and not a rigid one.

When the patient is standing on his head, pressure may change because many of the above-mentioned factors change, but nevertheless the pressure in the sacral part will be 40 cm. less than in the ventricles.

Assuming again a pressure of 30 cm. at the base of the spine, there will be a pressure of — 10 in the upper part (fig. 2). We now find an increased pressure in the ventricles. This fact does not give rise to severe disturbances as bloodpressure has also been increased by standing on the head, and preventing the bloodvessels in the skull from being compressed.

What happens now when air is inflated? In a normal case we keep the pressure at the base of the spine all the time at 30. If the air has penetrated the ventricles as far as the basis of the skull, pressure at the basis of the skull remains at 0 and also the total pressure in the ventricles (fig. 3). If the air penetrates to lower levels, e. g. halfway up the spine, we get a condition as shown in fig. 4. In case of a still lower level, pressure in the ventricles still increases; in cases of total displacement to an amount of 30. But now the conditions are quite different to those, which existed, when the patient was standing on his head, because now there is no increase in the bloodpressure and so the vessels are compressed, which is accompanied by an enlargement of the original liquor-space. This increase in pressure is in my opinion the cause of the troubles which may arise after spinal inflation.

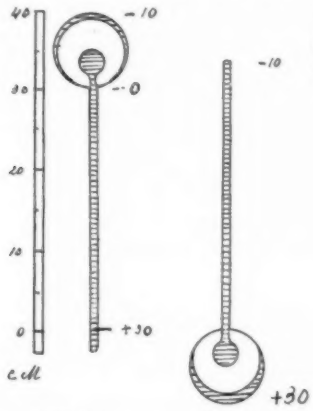

Fig. 1.

Fig. 2.

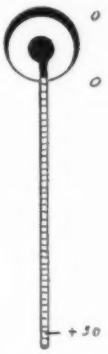

Fig. 3.

Fig. 4.

Fig. 5.

We do not observe the pulse becoming low and irregular until a definite quantity of air is inflated, the face becoming red owing to irritation of the vasomotornerves and violent perspiration and vomiting take place. After a while paralysis of the vasomotornerves is seen, the patient gets worse and sometimes faints. This state only gets better when the air is gradually absorbed and the pressure again becomes normal.

A second proof that we have to do with this kind of increased pressure, is the fact, that after inflation some cm.^{3} of liquor always seem to be missing. We have always explained this as being due either to leaking in the tubes, to liquor remaining in the tubes, to compression of the air or to absorption by the meninxes, but now it is clear, that we must explain the phenomenon as above.

The compressibility is of no importance because in case of pressures of 15—30 cm. the air only changes 1 à 2 ‰ in quantity. Knowing the cause, we will have to try to remove this drawback. This will be possible by preventing the air mounting higher than the occipital fossae. Perhaps this may be done in different ways.

1:o. By auscultation. The air is distinctly heard, when bubbling upwards. The bubbling in the occipital fossae will cease, when we have insufflated enough air.

2:o. By insufflating very slowly and stopping as soon as the face becomes flushed and vomiting begins.

3:o. By letting the liquor flow out of the spinal canal by means of a suboccipitalpuncture, so that the air can not come below the puncture.

We can also act in another way, viz. by taking care, that the quantity of the insufflated air never becomes more than the quantity of the liquor which has flown out. The manometer indicates pretty accurately how far the insufflation has succeeded. The decrease in pressure indicated by the manometer is almost proportional to the drop in the level of the liquor in the canalis cerebro-spinalis.

If we continue till the pressure drops to about 10 cm., we may assume, that the ventricles are completely insufflated. This fall in pressure will soon be attained in case of small ventricles or of a closed foramen MAGENDIE and of course much later in case of hydrocephalus internus.

One danger still remains, viz. the fall in pressure which may arise from there being no communication between the liquor in the skull and in the spine for this may endanger life by compression of the medulla oblongata. To avoid this, we have to take some precautions as:

1:o. not to use an open manometer but a closed one, thereby preventing loss of liquor.

2:o. to insufflate immediately some air, and thus start with slightlv increased pressure.

The occlusion as mentioned above will be evident as soon as a small quantity of liquor has run out; then we must stop. It is sufficient to know, that an occlusion exists.

Discussion. STENVERS points to the fact, that the insufflated air after being introduced into the ventricles, expanses owing to the temperature of the body being higher than that of the room. This fact has not been mentioned by speaker. He thinks it a mistake of speaker, to insufflate a quantity of air equal to that of the liquor which has flowed away. DANDEE reckons with this fact and therefore introduces a smaller quantity of air. As DANDEE told him personally, he often makes a puncture for the purpose of discharge, some time after the injection. And just as in the case of DANDEE's method it is also necessary in the BINGEL method, which has been followed by speaker, to inject a quantity of air smaller than that of the liquor which has flowed away. — He must emphatically warn against the application of encephalography in cases of tumors situated in the back part of the skull. It is not true, that the danger is apparent immediately after puncture, some time generally elapses before death takes place. It is not easy to explain these facts, as experiments of BOUWDYK BASTIAANSE show, where it was found that the pulse becomes slow only six hours after lumbarpuncture. STENVERS experienced the same phenomenon. Along with other factors, we must reckon on the liquor oozing out through the puncture aperture. We must not underrate this danger, because it may sometimes be extremely difficult to decide whether a tumor is in the fourth ventricle and so we may unconsciously run the risk of applying encephalography in such a case.

DIETZ thinks it very dangerous to apply theoretical views on pressure in a rigid system of tubes to conditions in the living body, where matters are quite different, as one has to do with elastic tissues, not to mention other differences which may exist. He himself has applied this method in a large number of cases and has read a paper about it before this society. Accord-

ing to his experience it is impossible to explain the symptoms by a simple increase of pressure. In order to eliminate this increase of pressure it is sufficient to make the patient lie down with the head low as soon as he does not feel well. He always applies this simple method. If speaker is right, all disagreeable symptoms would disappear; but they only decrease. So it seems to him that the question is much more complicated than it appears to speaker. Finally he is doubtful whether with good technic and sufficient experience the quantity of air insufflated, will ever be so great as to fill up half or more of the spinal canal, when the patient is in an upright position. Has the speaker ever actually seen this himself?

VAN EBBENHORST TENGBERGEN answers, that the expansion of the air caused by the difference of temperature in the body to that in the room, is so very small, that it may be regarded as negligible. He thinks the diagnostic value of this method great enough to justify us in running the risk of applying it to a case of tumors in the fourth ventricle. DIETZ is right in saying that a system of rigid tubes can not be compared with the body, but the influence of gravity is the same in both cases. He never saw the air filling up a large part of the canalis spinalis, but he thinks it does occur. All the disagreeable symptoms however do not disappear when the patient is placed with the head hanging downwards, because this position is only adopted when the symptoms caused by the increased pressure, have already appeared; and even after the cause has ceased, the result may continue.

II. L. G. HEILBRON. *Demonstration of a New Tubestand for Metalix Tubes.*

III. R J. HARRENSTEIN. *Radiological Study of the Hipjoint in Children.*

(Published in the Acta Radiologica. Vol. IV, 357.)

Discussion. DRIESSEN asks why the angle formed by the two γcartilages becomes larger when the child gets older. It may perhaps be explained by gravitv causing a drop of the pelvis in relation to the femur.

HARRENSTEIN answers, that he also has been thinking over the reason of the change in the direction of the γdisc. But he has not been able to solve the problem.

IV. L. G. HEILBRON. *Annular Shadows of the Pleura.*

It is a well known fact, that cavities, which cannot be found either by percussion or auscultation may occur in the lungs. The greater part of these cavities may be found radiologically. In relation to prognosis, cavities are of great importance. In our country patients with tuberculosis of the lungs, showing cavities, are not considered suitabe for treatment at the expense of the Invalidity Found. For that reason it is a matter for regret, that the radiologist sometimes diagnoses cavities, where none exists. We now know, from the publications of DAHLSTEDT and others, that some of the annular shadows seen in the lungs are not cavities. Some of them disappear after some time. TURBAN thinks, that cavities in tuberculous lungs also may disappear by being cured. Different authors give radiological signs to differentiate the so-called annular shadows of the pleura from cavities.

Speaker demonstrates 3 cases of radiographs of the lungs of patients, with no clinical or physical signs of cavities, showing typical annular shadows.

After introduction of lipiodol these shadows nevertheless turn out to be cavities, communicating with the bronchi.

Perhaps we cannot conclude from this experiment that our cases are real intrapulmonar cavities, as they may only be cases of local open pneumothorax.

Discussion. MORTIER HYMANS points out the fact, that a distinct »bronche de drainage» is important for the differentiation between cavities and local pneumothoraces.

DIETZ observes that in the demonstrated radiographs, the size of the lipiodol shadow is smaller than the size of the annular shadow, seen before the introduction of lipiodol. If this fact can be explained by saying, that the lipiodol only partially fills up the cavities, why then is the lipiodol shadow quite circular and of the same intensity all over?

VAN EBBENHORST TENGBERGEN should like to know, if speaker has not tried to get more proofs of the situs of the shadow by stereoscopical examination.

HEILBRON thinks the value of the »bronche de drainage» very small. He explains the smaller size and circular outline of the lipiodol shadow by the lying down position of the patient. Stereoscopical examination is another source of mistakes, as it is difficult to be quite sure that both heart and respiration have been in the same phase during the whole examination.

V. H. TIMMER. *The Lack of Sharpness in the Radiological Image and a Simple Way to Fix the Size of the Focus of an X-Ray tube.* Published in Fortschritte a/d Geb. d. Röntgenstr. Bd 33, 769.)

Discussion. HEILBRON observes that the radiographs of TIMMER made at a focus-plate-distance of 60 cm. prove, that the focus of his tube is too large.

TIMMER answers, that a focus of 3 mm. diameter according to the opinion of others is a small focus.

BOUWERS has measured the size of foci of different tubes. Whilst it has always been thought, that a focus of 3 mm. was a very large one, the smallest focus he could find had a diameter of 3,1 mm. Only the »Mediatube» has a focus of 2,2 mm. measured at an angle of 30°; making the angle larger we very soon find 3,5 mm. He uses the pinholecameramethod.

BRONKHORST observes. that with the same tube, using a pinholecamera, we may find a diameter of 3 or of 0,5 mm. With a short exposure we may find a small and with a long exposure a large focus. The radiation-energy is strongest in the centre of the focus. This part of the focus he calls the effective focus. The effective focus may be only 1,2 mm.

TIMMER asks how BOUWERS measured the focus with the pinhole camera. FRANK (Hamburg) rejects this method.

BOUWERS does not think it is good to divide the focus into stronger and weaker parts, but thinks it is better to indicate the size of the focus by its diameter. It is true, that with long exposure, the outerpart of the focus becomes more important. He gives a short description of the pinholecamera-method.

VAN EBBENHORST TENGBERGEN observes that TIMMER's method does not show any important difference from the pinholecameramethod.

VI. A. BOUWERS. *New types of metaltubes.*
(Published in Fortschritte a/d Geb. d. Röntgenstr. Bd 33, 575.)

Fig. I.

Fig. II.

Fig. III.

Fig. IV.

Fig. V.

Fig. VI.

Fig. VII.

Fig. VIII.

Fig. IX.

Fig. X.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 1.

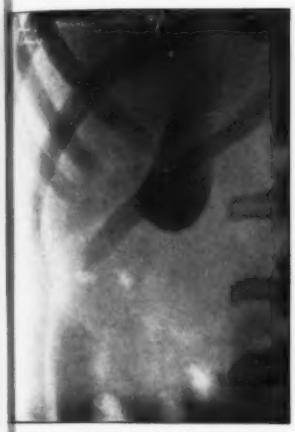

Fig. 1. Pt. Nr. 16. Normal gallbladder shadow.

Fig. 2. Pt. Nr. 4. Normal gallbladder shadow.

Fig. 3. Gallbladder shadow from the same patient (Nr. 4.), taken one hour after Fig. 2 and after intake of bread and butter.

Fig. 4. Pt. Nr. 12. Normal gallbladder shadow, elongated.

Fig. 5. Pt. Nr 7. Normal gallbladder shadow, deep-lying.

Fig. 6 Pt. Nr. 3. Normal gallbladder shadow, high-lying.

Fig. 7. Pt. Nr. 20. Normal gallbladder shadow; barium-meal in ventricle and duodenum.

Fig. 8. Pt. Nr. 36. S-shaped gallbladder shadow with numerous negative stone shadows.

Fig. 9. Gallbladder from the same patient (Nr. 36) removed at operation.